

3 1761 06383281 0

THE GREAT LIBEL CASE.

DR. HUNTER

1826-

VERSUS

PALL MALL GAZETTE.

BEING A

VERBATIM REPORT

OF

THE MEDICAL EVIDENCE

GIVEN BY

DR. WILLIAMS,
DR. RISDON BENNETT,
DR. ORLANDO MARKHAM,
DR. GEORGE JOHNSON,

DR. COTTON,
DR. RICHARD QUAIN,
AND
DR. ODLING.

SHOWING THEIR OPINIONS ON

THE NATURE, CAUSES, AND CURE OF CONSUMPTION,

WITH

EXPLANATORY REMARKS BY

DR. H

F.

RC
310
.5
G73
1867
.1
GERSTEIN

MITCHELL & CO

(Published

FLEET ST.

Handle with
EXTREME CARE
This volume is
BRITTLE
and cannot be repaired

Photocopy only if necessary
GERSTEIN SCIENCE
INFORMATION CENTRE



Library
of the
Academy of Medicine
Toronto
25902

1936

INTRODUCTION.

THE principal objects I have in view in publishing a verbatim report of this trial, are—1st, to expose the *errors* and *misrepresentations* committed by the so-called “*scientific witnesses*” of the defence, in my action against the *Pall Mall Gazette* for the publication of a *base* and *malicious* libel on my character; 2nd, to point out the *fallacy* of the reasoning adopted and the arguments employed by the Lord Chief Justice in his charge to the Jury; and 3rd, to express the indignation I feel at the great injustice done me by the verdict rendered.

No person, not biassed by *prejudice*, or influenced by *interest*, can review the course adopted by the defendant on the trial without arriving at the conclusion that not one tittle of evidence was produced by him which can justify or even palliate the infamous accusations which led to the action.

I. He charged me with putting forward false pretensions as to my professional qualifications. I met this by producing my diplomas and credentials in Court, and by testifying on oath to the nature and extent of my professional studies, as well as the schools at which my diplomas were obtained. No attempt was made by the defendant to call in question their value; to throw a doubt upon their genuineness; or to shake the credibility of my evidence as to the ample nature of my medical

education, and the extent of my subsequent professional experience. His complete silence on these points was a tacit admission that all his shameless insinuations were not only false in fact, but that he *deliberately* made them without the least foundation.

II. He accused me of seeking to induce the public to believe that my medical qualifications had been obtained in England. This was fully disproved by the fact that on the title-page of every edition of my book I am described as "Doctor of Medicine of the University of New York, and Licentiate in Medicine and Surgery of Canada;" and by the preface written by Dr. MacGregor, in which I am spoken of as "Dr. Hunter of New York." In spite of this clear documentary evidence, a faint attempt at justification was made during my examination, on the ground that in the signature appended to the letters extracted from my book by my publisher, and incorporated with the advertisement of it in the newspapers, the simple initials "M.D." followed my name. But it is matter of common notoriety that no person ever thinks of superadding the name of his university to the symbol of his degree, be he D.D., LL.D., M.D., M.A., B.A., or M.B., unless for certain official purposes, or on the title-page of a book. This forensic "feint of cunning fence" was, however, shown to be an utter *sham* by the fact that nobody could be produced who *had* mistaken the source of my professional qualifications, and hence had to be abandoned.

So also the adroit insinuation involved in the question as to whether I was not aware that it was *penal* to style myself M.D., unless the degree had been obtained in England! The defendant's counsel must have known

full well when he tried this *feint*, that the statute under which even a graduate of Edinburgh had been subjected to *fine* was long ago repealed, and that under the existing Medical Act I have as valid a right to the use of my collegiate titles and to practise my profession as any physician in England, the only restrictions being that I cannot register my qualifications, cannot hold certain official appointments, and cannot collect my fees in the courts. All this was admitted by the Lord Chief Justice at the time.

III. He *insinuated* that the foul charge made against me (and which at the time of publishing the libel had not even been investigated before the magistrate) was probably true, thereby endeavouring to *incite* against me such prejudice in the public mind as to render it impossible to impanel a jury which might not have been biassed by his words. I have twice denied upon oath that there was a shadow of foundation for such a charge. No attempt was made by the defendant to justify this insinuation, to question the justice of my prompt acquittal, or to rehabilitate the charge in any form. On this point the Chief Justice said that if the Jury believed he had *meant* to impute my guilt, which he did not think was the case, then there was not a particle of evidence to justify such imputation.

IV. Under this head I will include the charges of "inveigling" and "terrifying" persons to become my patients, of "deluding" them with the hope of being able to cure them, when I knew that I could not do so; of "extorting enormous fees," and of "*malpractice*." In answer to this, *twelve* gentlemen came forward and stated in open court that they had been my patients; that they

had suffered from various diseases of the chest, including every class mentioned in my book; that they had derived great benefit from my treatment, and were entirely satisfied with me, the fees, and the results. On the other hand, not a single person who had been under my professional care could be put into the witness-box by the defence, to say that he or she had been misled, had suffered extortion, or had adopted the treatment and been "*made worse, or no better.*" On each and all of these points the action was to all intents and purposes *an undefended action!*

When it is considered that this action had been pending a whole year—that it was admitted by the medical witnesses for the defence that they had been in communication with many of my patients—that the defendant had evidently kept up a system of *espionage* upon my practice—and that I had been twenty-six months in practice in London, and nearly *two thousand* persons had availed themselves of my professional services, yet not one could be put into the witness-box to say that he had been *inveigled, deluded, or maltreated* by me, or had suffered any injustice at my hands. If the defendant could have obtained *one* such person, there can be no doubt he would have done so. I question whether there are many practices in London, embracing an equal number of cases, that would have borne a similar test.

These embrace *all the points of the libel of which I complained*, and for which I sought redress. The defendant had no defence to offer. He was not able to justify even the least important of them, yet, strange as it may seem, his Lordship studiously ignored the palpable fact

that the defendant had failed on every point to make out even a colourable justification, and in his charge to the Jury even introduced into his *argument* for the defendant many points which had eluded the legal acumen of the defendant's counsel.

I now come to the evidence of the defence, which was an entire surprise, inasmuch as it was believed by me, and regarded by my able counsel, as totally *irrelevant* to the case, and *legally* inadmissible. The defendant *practically* admitted that every word he had uttered was *false*, but then he said, here are *six* medical men willing to say that they do not believe your *theory*, and are prepared to disparage upon oath your *descriptions* of disease, and on their evidence I rely for my defence! Such evidence, I contend, had no bearing upon the case, and never would have been *offered* or *received* against an English physician. It was impossible that my counsel could be prepared to make a suitable cross-examination in abstruse *physiological*, *chemical*, and *pathological* questions, or could know what value to attach to the answers given, or when the answers were truthful and when evasive. Had I contemplated such a *trick* as this, I should have been my own counsel, and nothing, I can assure the so-called *scientific* witnesses for the defence, would have afforded me greater pleasure than to have had the opportunity of conducting their cross-examination in the public court.

Then the Judge and the Jury were in the same state of helplessness, for they neither *did nor could understand* half the evidence that was given. With all his Lordship's admitted ability, the extraordinary misconstructions he put upon the evidence, and subsequently served

up to the Jury, are such as to expose the administration of British justice to the criticism of the world. It was lending the influence of the Court to a medical clique, and enabling that clique to make use of its authority for their own purposes.

But enough. Now a word as to the evidence of the medical experts. My book was intended for popular instruction, and hence did not pretend to deal in abstruse and unintelligible technicalities. In writing for the public, to have done so would have been to render the book worthless, for few are able to draw nice distinctions; and it is impossible to explain many medical terms without burdening the matter. Hence it was that, in speaking of tubercle, I spoke of it in general terms as "carbon" and "*carbonaceous*." This was not *literally* true, because carbon exists in its pure state only in the diamond. But I did this designedly, to avoid the use of such confused terms as *hydro-carbon*, *cholesterine*, *carbonic acid*, *carbonic oxide*, &c., which would need explanation, and even then could hardly be rendered intelligible. This point the medical witnesses fastened upon, and on this shallow pretence—construing me literally, and refusing to take me as they knew I intended to be understood by the profession—built up the whole fabric of their evidence on the nature of tubercle, and on the action of oxygen upon it. Their evidence was wholly *ex parte*, and I had no adequate means of exposing the nature of this *disingenuous sham*; and hence it went to the Jury as proof that I had misrepresented the matter.

Regarding "*the chemist's*" statements, I have, in my notes appended to his evidence, shown that he was on

many points grossly ignorant of that which he professed to understand; and that, even when correct, the matter either had no bearing on the case, or did not warrant the construction put upon it. On one point, which I regard as material, I have offered to submit the truth of his testimony to a practical test, which he can hardly refuse without exposing himself to the imputation of having wilfully misrepresented a fact.

The *plot* of the medical evidence seems to have been that each of the medical men should select some form of *inorganic carbon*, entirely different in its nature and properties from any form of carbon which exists in the body; and then, first deny that "*as such*" it existed in tubercle; and then deny, if it did so exist, that oxygen could act upon it in the manner represented by me.

It is matter of common knowledge that experts are not compelled by legal process to give their *opinion* in courts of law in civil suits. They do so for compensation, or voluntarily as a matter of direct personal interest in the cause of either litigant. Here then we see several medical men combining to throw the weight of their professional reputation into the scale against a member of their own profession; and the question naturally suggests itself to the mind, with what motive? If it was to combat what they conscientiously thought to be an error of theory and a mistake in practice, surely a more ordinary field of opposition was open to each and all of them through the press; by whose instrumentality they could have submitted their views to a tribunal adapted by education to entertain the several questions at issue, and to decide upon the merits of such a controversy.

What did it matter to them whether the proprietary of a daily journal had to pay a few hundred pounds for a libellous article, unless, indeed, they were pecuniarily interested in the issue of the action itself; while, on the other hand, they must have been conscious that the result of their combined exertions, if successful in the manner in which these were made, might compass the destruction of my social and professional status; an object so utterly *base* and unworthy the desire of high-minded members of a liberal profession, that it is only in the absence of any other apparent benefits to be gained, that I can bring myself to admit the probability of such a motive. It will be for the public now to determine whether the science of medicine has gained by the course adopted by these witnesses, or whether they have not rather revealed the existence of a disgraceful cabal, and exposed a clumsy organisation for the accomplishment of its end.

Lastly, I have shown that disease is more fatal *now*, than it has been at any former period within the past *thirty* years, and that the chief increase in *fatality* is in *diseases of the Chest*; that so far from *Cod-liver oil* having diminished the number of deaths occurring in a given population, they are greater under its use than they were before its introduction! These are stern facts, and of far greater consequence to mankind than a mere squabble among doctors about theories.

A word about my descriptions of disease. I have shown that on every point extracted, my language is in no respect *stronger* than that held by Sir James Clark, Sir Thomas Watson, and other great lights of medicine in England. If Consumption is to be *prevented*, or its

fatality diminished in any sensible degree, it can only be effected by cutting off those more curable maladies which serve to develop and feed the pulmonary disease; and by inducing the afflicted to avail themselves of medical aid before it has passed the *early stage*. My work aimed at this, just as Sir James Clark's did years ago. The course pursued by my enemies is as suicidal for the profession, as it will assuredly prove disastrous to the public, if their advice be followed. These matters have now become of very secondary consequence to me, but I wish it to be distinctly understood that whatever I have written on this subject, has been put forward with an earnest and conscientious desire to discharge my duty to my fellow man. I have every right to complain that I have not had *justice* or *fair play* meted out to me in England, but I doubt not I shall be able to obtain, by constitutional means, or natural law, that justice which, as a Canadian physician, I find it vain to expect from the judicial tribunals of my country.

ROBERT HUNTER, M.D.

14, Upper Seymour St., Portman Sq., London.

January 8th, 1867.

A London physician of large experience of the journals and hospitals has kindly sent me the following note, with liberty to publish it.—R. H.

“ . . . I must honestly say that I am very much ashamed of the fact that half-a-dozen medical men could be got on the slender pretence of some assumed superiority of method of curing tubercle in London, and some supposed breach of medical etiquette in advertising (when all of them advertise themselves, and our medical journals are PARTIALLY supported by empirical advertisements). . . . It speaks badly for that supposed free trade in physic, or professed honesty with which we hold out our hand to our American brethren. We want consistency and largeness of vision in our great medical guides.

“ I believe it to be a great benefit, that somebody like yourself occasionally comes to stir up the stagnant pool of selfishness and obstruction which besets everything medical in London, where the interests of the sick are really in question. We are eaten up with selfish trade jealousies in journalism, and yet almost everything medical that appears in the newspapers, whether as to water supply, and castor-oil and cholera, deficient cubic space in workhouses, the nature of the cattle plague, &c., is just as much misrepresented as your views of the value of inhalation in chest diseases. The very statistics of the Brompton Consumption Hospital support your views, and those of Dr. MacCormac, that carbon is at the root of tubercle.

“ I perceive also that the organs of public opinion (as

they are called), the chief ones let us term them—the *Times*, &c.—are very decided that the carbon or carbonaceous theory of tubercle condemns you; that neither carbon nor scrofula has anything to do with consumption, and as Dr. Risdon Bennett or some witness holds, it is rather a chimera to try to cure consumption. I remember, however, with grief and misgiving, that exactly the same organs (with the honourable exception of one morning paper) were equally decided, not many weeks ago, that it was a chimera to say rinderpest could have been imported, but that it was manufactured (in the direful wisdom of that time) by bad sanitary arrangements in London cow-houses; that vaccination was the real and perfect cure, and that Professor Gamgee was to be condemned, who thought the plague was infectious, as this idea would suggest such a thing as quarantine.

“I remember that almost exactly the same phrases employed in condemnation of carbon in tubercle, were used in a notable trial where an innocent man was condemned by the leading newspapers for poisoning his wife with antimony and arsenic; and yet the antimony (as proved by Sir B. Brodie) turned out to be a harmless metal (bismuth) taken medicinally, and the arsenic was, in reality, extracted from the infallible tests of the police-office toxicologist; but just as the *Times* never altered its opinion about that case, so it will not alter its stereotyped enigma now—that carbon or scrofula has nothing to do with phthisis, and such carbon must be burnt only like a candle.

“I remember, too, that, in addition to this enlightened toxicology and cattle plague pathology, we have had similar leading articles to prove, from a theory (now

proved at Guy's and other hospitals to be quite childish) that castor-oil and purgatives are the true remedy for cholera, as the purging and vomiting are efforts of nature to cure the patient!

"I remark also, with regret that a dozen medical men and sober journalists have, in every way, impeded such vast improvements as ovariotomy, anæsthetics in midwifery, the supporting treatment in pneumonia and typhus in place of mercury and the lancet (blood taken in quarts); who have opposed iridectomy, and, in fact, almost every modern medical or surgical improvement; who, I happen to know, would now bleed in typhus or pneumonia, salivate for other forms of disease, &c., and are so truly conservative as to conspire against inhalations in phthisis or asthma.

"I would implore the thinking public not to be seduced by such guides, or the heap of errors in Judge Cockburn's charge; we have had all this before in the cases just mentioned. Our lawyers, juries, and press have also been wrong about the medical questions involved in such cases as that in which antimony was mistaken for bismuth.

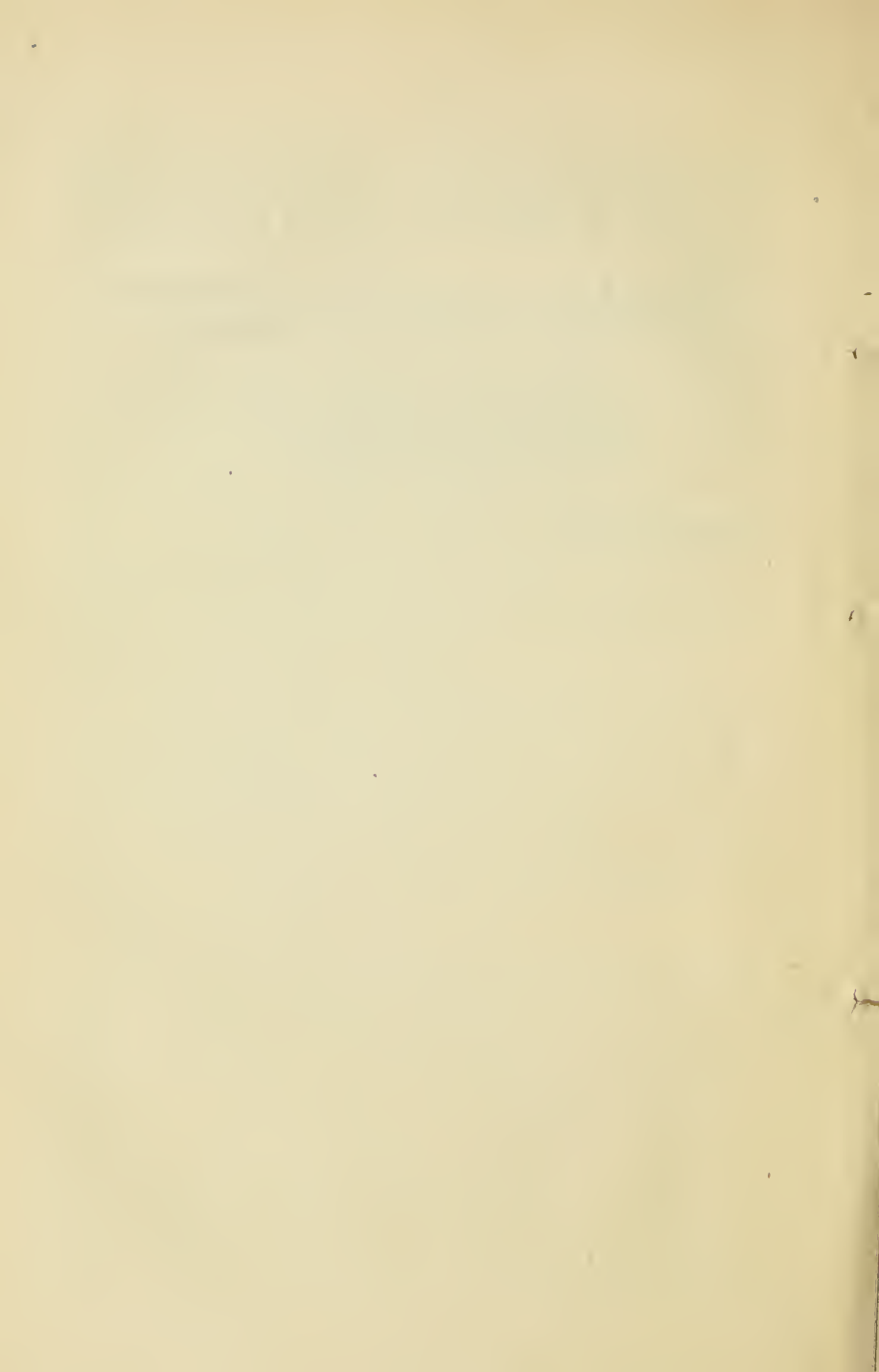
"The eminent chemist Liebig has founded a school, of course, on this theory of yours of the combustion (vital force) of carbon (it need not be lighted as a candle, as wisely promulgated). It seems deplorable that all the leading witnesses (as they state themselves of Sir J. Clark) have no faith whatever in medicine in consumption, and yet we know that in at least two out of its three stages, inhalations, counter-irritants, cod-liver oil, &c., are most valuable when conscientiously and skilfully applied. In this point of view I believe you are doing

good ; indeed, the entire subject of inhalations and pulverised fluids has opened up a new field in the cure of lung diseases, of which the older men knew next to nothing.

“ I fear, in fact, it is the old story, crystallised for us in verse by Hudibras, as to doctors and lawyers—

“ ‘ Anatomists dissect and mangle,
To cut themselves out work to wrangle ;
But lawyers are too wise a nation
To expose their trade to disputation.’

“ *January 3rd, 1867.*”



SECOND DAY.

DR. ROBERT HUNTER, *sworn*.

Examined by Mr. SERJEANT PARRY.

I believe you are a Doctor of Medicine of the University of New York?—I am.

And a Licentiate of the Medical Board of Canada?—I am.

Will you produce those two diplomas?

[They were produced by the witness.]

Mr. Serjeant Parry. What is the date of the diploma of the New York University?

The Associate. 1846.

Mr. Serjeant Parry. And of the Licence?

The Associate. 7th August, 1846.

Mr. Serjeant Parry. Your father, I believe, Dr. Hunter, was a Doctor of Medicine, and a native of this country?—Yes.

I believe you were born in this country?—Yes.

Did your father at an early age, during your youth, go to a State in Canada?—He emigrated to Canada in 1826.

How old were you then?—About six weeks, I believe.

At all events, you were born in this country?—Yes.

Did your father practise as a Doctor of Medicine, and as a general practitioner in Canada?—He did.

For how many years?—For about twenty years.

And is he still living?—He is still living.

He has retired from the profession?—He retired from the profession in 1846.

Were you educated for the medical profession by him?—I was.

And any of your brothers?—Two of my brothers.

And are your brothers medical men now?—Yes.

Practising where?—Practising in Canada. One of them, just at the present time, I believe, is not practising, for he has retired.

But they have practised as medical men?—They practised up to the time I came to England, and one is still practising.

Were you educated for the medical profession?—I was educated in the first place under my father, in Canada. I commenced studying the profession in 1840; and in 1843, I think it was, I went to the University of Geneva.

That was the University of Geneva in the United States?—Yes, Geneva in the United States.

Is that University a recognised and distinguished school of medicine in the United States?—At that time it was one of the most celebrated.

How long did you study there?—I was two years at the University of Geneva.

During that time did you go through the regular education of a medical student?—I did, in all branches of medicine.

Did you attend the hospitals whilst you were there?—I did.

Did you attend the lectures upon various branches of medical science?—I did upon all branches.

I presume, as you were only there two years, that you did not pass any examination there. Did you, or not?—I passed no examination.

Where did you go afterwards to complete your studies?—From Geneva I went to New York.

Is the University of New York a distinguished school of medicine in the United States?—I believe the University of New York to be the most distinguished school of medicine in America.

May we take it that one reason why you went to America to study was that there were not the same facilities for study in Canada?—Yes; the facilities for studying in Canada were very small, recently established; and the teachers, again, were not of corresponding eminence.

Who was the president of the University of New York at the time you were pursuing your studies there?—Dr. Valentine Mott. I am not certain whether he was at that particular time the president.

Was he a teacher at that time there?—He was a teacher; but I think the presidency is held in rotation.

Similar to our College of Physicians; it is either annual or bi-annual?—Yes; that is so.

However, was Dr. Mott one of the teachers there?—Dr. Mott was Professor of Surgery.

I believe he signed your diploma, did he not?—He did.

How long did you stay in New York? Will you state the curriculum you went through there?—The curriculum was precisely the same as at Geneva. It consisted of six principal branches: anatomy—

The Lord Chief Justice. I do not think it is necessary to go into that.

Mr. Serjeant Parry. What degree did you take finally at that university?—Doctor of Medicine.

Did you pass through an examination in the various branches of medicine?—I passed through an examination in each separate branch.

By whom is your diploma signed?—The Chancellor of the University. [The witness read the names from the diploma.]

And you were examined by all those gentlemen?—I was examined by all of them.

When was this?—In 1846.

Where did you, then, begin your practice; in America or Canada?—I returned to Canada, and passed the Colonial Board, and took the colonial licence immediately after.

Did you before you obtained this license in Canada go through the regular examination which all regular medical practitioners have to do ? [Handing the diploma to the witness.] Is that the regular diploma signed by the Earl of Cathcart?—Yes; signed by the Earl of Cathcart, the then Governor-General.

And is it signed by any of the medical authorities?—There is the endorsement on the certificate of the Board of Medical Examination.

Is that *the* Board in Canada?—It is *the* Board. I was examined in chemistry, *Materia Medica*, and the diseases of women and children.

Have you in the course of your studies taken any other degree?—I have taken the diploma of the School of Medicine and Surgery.

Where?—In New York; but that is regarded, however, merely as educational.

That was merely a preliminary sort of examination, I presume?—No; it was not that; but it was rather in the form of a certificate of education, and did not carry with it either a title or the privilege to practise.

It was a voluntary examination, I presume, then?—It was the same as any of the certificates in the schools in London would be.

Have you that certificate with you?—Yes, I have.

Did you practise in Canada?—I practised in Canada five years.

Where in Canada?—Near the city of Toronto.

Had you a fair practice?—I had a very large practice.

At that time did you practise generally?—I practised generally all branches of the profession.

You did not confine yourself to any branch of the profession of medical science?—To no branch of the medical profession. I was a general medical practitioner.

At the end of five years I believe you were overworked, and obliged to give up your practice for a time?—I had an attack of spitting of blood, probably brought on by exposure and overwork, and I found it advisable and necessary to give up practice for a time.

I think you transferred it to your brother-in-law?—Yes.

Who was an English graduate?—An English physician, formerly house surgeon, I think, of the Hanwell Lunatic Asylum.

You gave up your practice in 1851?—Yes.

What did you do then; did you travel, or what?—In the first place?

I do not want to take you through all the particulars. How long were you away from practice? What interval elapsed before you resumed practice of any kind?—Perhaps about eight or nine months.

During that time did you apply yourself particularly to studying the causes of consumption and diseases of the throat?—Naturally, as I was labouring under it; and I not only did that, but I consulted all those professors of New York who were considered to be celebrated in diseases of that kind.

In order to carry out your intention, I believe you practised upon yourself, did you not?—I did.

Did you practise the system of inhalation which you profess still to practise?—I did; and I submitted the question of the possibility of inhaling oxygen to the professors of the University of New York, Mr. Draper, at that time, and he endeavoured to dissuade me from the experiment.

The Lord Chief Justice. Was that your own notion?—At that time it was entirely my own. I was not aware of any work that had discussed the subject.

Is Mr. Draper a professor of chemistry?—A professor of chemistry in New York.

Why did he endeavour to dissuade you?—He was under the opinion that the action of the protoxide of nitrogen, or laughing gas, which is one equivalent of oxygen and one of nitrogen, would make it a dangerous experiment in my condition; and as there was no experience to guide us, he advised me against it.

Mr. Serjeant Parry. Did you persevere in the treatment yourself?—Yes.

Although his view was not favourable?—As it involved the giving up altogether of restoration to health, inasmuch as the opinion expressed by Dr. Swett, who has been a very distinguished authority, and was one of the physicians of the New York Hospital, was that I was labouring under consumption, and that I should go to the South for change of air, and that medicine could really do me no good, it involved the abandonment of all hope of restoration.

The Lord Chief Justice. Well, you went on with the treatment?—I went on with the treatment.

Mr. Serjeant Parry. And did you find you derived any benefit from that treatment?—I gradually improved under it, and ultimately recovered my health.

Since then have you been subject to spitting of blood?—I have had spitting of blood on one occasion since, and retired from practice in consequence of it.

You again treated yourself in the same way?—I again treated myself in the same way.

When was that?—That was in 1857, I think.

That formed another interval of how long before you resumed?—About eighteen months, or two years.

And having found the treatment successful, you again resumed your practice?—I found the treatment sufficient to remove the symptoms. I may say that at that time I came to England and visited the Consumption Hospital at Brompton, and an institution at Lyons, in France, for the purpose of witnessing the treatment pursued at different institutions.

Did you find the treatment of inhalation prevailing at either of them?—To a very moderate extent at the Brompton Hospital, and at

Lyons they were using compressed air with the view of increasing the volume of oxygen received at each breath.

Having visited Europe, you say the Brompton Hospital and the hospital at Lyons, did you then return to New York?—I then returned to New York and resumed my practice.

The Lord Chief Justice. You resumed your practice?—I resumed my practice.

How long were you in Europe?—Oh, about two months, I think, only.

Mr. Serjeant Parry. He said he was away at this time about eighteen months.

The Witness. Well, I think that is incorrect as to the time. I think it was after my return to New York, and after my resumption of practice for a short time, that I then gave up the practice for eighteen months.

The Lord Chief Justice. What was the occasion of that?—The occasion was, I felt the close confinement and the labour of the practice on my health, having had a relapse of the spitting of blood. My consultation practice, which was confined entirely to the house, is considered by all authorities on consumption, I believe, to be particularly unfavourable to those suffering under maladies of that kind. It certainly is my opinion.

Mr. Serjeant Parry. But you did afterwards resume your practice?—I did.

When you considered yourself cured?—Perfectly cured. I regained my health perfectly.

I believe you have never had any symptoms since, have you?—I have never had any decided symptoms since. My family are consumptive, that was the reason of my father emigrating to Canada, and both my brothers have had attacks of spitting of blood, and both of them have had to resort to inhalations as a means of relief.

And those are the causes which attracted your attention to, and have concentrated your attention on this matter?—Naturally, it would do so.

How long did you continue in New York devoting yourself, I believe, solely to this disease?—From 1852 to 1857, and then at intervals since that time, perhaps altogether eight or nine years.

Had you a very considerable practice?—I had a very large practice in New York.

And you confined your practice to this disease?—Diseases of the chest.

During that time had you published any work on consumption?—Yes, I had published two works on consumption.

Tell me what they were. Is this one of them which I hold in my hand, the 5th Edition?—No.

The Lord Chief Justice. When did you publish this work?—In 1853, I think, the principal work was published, and the other was merely a pamphlet, published in 1852 or 1851, I think.

Mr. Serjeant Parry. Did you also edit a medical publication?—I did.

I believe that was exclusively devoted to considering these diseases of the chest?—It was not exclusively devoted to that.

But mainly so?—The chief object. There were other contributions on matters of medicine.

When you arrived in England, had you come direct from your practice in New York?—No; I came direct from Canada to England. I had gone over to Canada from New York.

Had you practised in Canada?—I had practised in Canada for the last year that immediately preceded.

Was that the year 1863?—That would be the year 1863, up to June, 1864.

On the 27th of June, 1864, did you arrive in England?—I did.

The Lord Chief Justice. What made you give up the Canada practice?—I had no reason to give it up, my lord, except a desire to come to England for the education of my family, and to visit relatives.

Health had nothing to do with it?—Health had something to do with it, probably.

Mr. Serjeant Parry. At that time were you a man of independent means, apart from your profession?—Well, moderate means.

Did you bring your wife and family with you?—I did.

How many children have you?—I have eight children living.

And have they resided with you since you have been in England?—They have resided with me entirely.

When you came to England, did you seek to establish yourself here as a practitioner of medicine?—I did.

Before you came, were you aware of the Provisions of the Medical Registration Act of 1858, with which you have since become acquainted?—I was not.

We cannot go into the details of any correspondence you have had, but I may ask the fact, did you apply to be registered?—I did apply to be registered, and, on coming to England, I supposed that I had only to present my credentials, especially from Canada.

Did you also correspond with Dr. Burrowes, the President of the Medical Council?—I did.

On the subject of your being registered?—Yes, on the subject of my being registered.

Did you also write to Sir George Grey, who was then Home Secretary, on the same subject?—I did.

I believe it is a fact that you have ascertained that if you had practised in England, under these diplomas, before 1858, you would have been entitled to register?—I should been entitled to register.

Without any fresh examination?—Without any examination, upon my diplomas.

When you came over to England did you publish a work upon consumption, a copy of which I hold in my hand?—I did.

I have the 5th Edition of them. I will read the title, if you will allow me: "Practical Letters on the Nature, Causes, and Cure of Catarrh, Sore Throat, Bronchitis, Asthma, and Consumption, with Cases. By Robert Hunter, Doctor of Medicine, University of New York," and so on. In practising in New York, and since you have confined your attention to diseases of the chest, did you associate yourself with any other practitioner?—I associated myself with Dr. McGregor before commencing practice.

The Lord Chief Justice. Are you speaking of New York or London?—London.

Mr. Serjeant Parry. I was asking, while practising in New York, did you associate yourself with another Doctor of Medicine or medical practitioner?—I did with several.

Did you consider that essential for carrying out your system of inhalation?—That was rather due to the extent of the practice than any other person.

Was a gentleman of the name of Dr. Melville, who edits this work, a gentleman with whom you were associated in New York?—Yes.

And in Canada?—No.

Only New York?—Only New York.

For how long was he associated with you?—From 1854, I think, till the termination of my practice there.

For many years?—For many years.

When you came to London, did you associate yourself with a gentleman of the name of Dr. McGregor?—I did.

Was he a Doctor of Medicine of Edinburgh?—He was a Doctor of Medicine of Edinburgh, and a Member of the College of Surgeons, I think.

You ascertained that it was not illegal to practise in this country without registration?—I had that opinion given, but that I could not recover my fees, and that I had not a right to hold certain appointments.

The Lord Chief Justice. You had a right to practice, but you would not be able to recover your fees.—Not in Court; and that I could not hold certain medical appointments.

Mr. Serjeant Parry. I believe you ascertained that also from Dr. Burrowes that you were not debarred from practice?

Mr. Karlake. I must object to the question.

Mr. Serjeant Parry. I only want to show his *bona fides*. You say he is a scoundrel and an inposter.

At all events, you took a legal opinion to practise?—Yes.

In practising here in London, were you successful?—I obtained a very large practice.

I believe a great many of your patients are here?—Yes.

They have been subpœnaed by you to attend this trial?—So I believe.

I will ask you now about this book. We see it has passed through

five editions; and that Mitchell and Co., of Red Lion-court, Fleet-street, are your publishers?—Yes.

Did you give them directions to advertise this work?—I gave them instructions to advertise it.

And it was advertised in the way Mr. Coleridge described?—Yes.

The Lord Chief Justice. In order that we may fully understand the case in all its bearings, let us know what the advertisements were.

Mr. Serjeant Parry. There is no intention to keep them back. I intended to put one of them into this gentleman's hands. I propose to show that he did advertise first, and then put in one, or as many as we have, to show the gentlemen of the jury what the exact nature of the advertisement was.

Did you advertise in several newspapers, and amongst others in the *Times* newspaper?—The first advertisements were inserted in the *Times*.

You did not confine yourself to cheap newspapers, because the libel refers to that?—No.

I have in my hand the *Times* of September 28th, 1864. I am not sure that this was the first advertisement, but it gives the form of it. I will place that in your hand. You were aware of these advertisements being published, and they were published by your authority and direction?—Yes.

Is that an advertisement published by your authority?—Published with my sanction.

That is the same thing.—But I should say that this is not the form in which the advertisements were commenced.

The Lord Chief Justice. Let me see that. [It was handed to his lordship.] I think that is the form in which most people have seen it. I recollect it at the time.

The Witness. I think your lordship hardly understood the purport of my observation.

Mr. Serjeant Parry. We wish you to explain.—The instructions given to my publisher was to publish the advertisements giving the title of the book, my degrees, Dr. McGregor's degrees, and Dr. McGregor's preface explaining who I was, and then to fill up the column with an extract from the book.

The Lord Chief Justice. That is the form in which it was generally advertised?—That is the form in which it was continued. It embodied a mere extract from the book itself.

That is the form of advertisement that was in circulation at the time the *Pall Mall Gazette* article was written?—Well, I presume so.

The Associate. "Just published, price 2s. 6d."

The Lord Chief Justice. Each advertisement contains a certain number of letters?

Mr. Karlake. Yes.

Mr. Serjeant Parry. Was the whole book advertised?—I cannot answer that question.

Nearly the whole?—I believe nearly the whole.

The Lord Chief Justice. Who made the extracts?—Mr. Mitchell entirely.

The publisher?—Yes; the first part, the advertisement, and then he made the extracts.

The Lord Chief Justice. The jury may wish to see the form of advertisements.

A Juror. We are quite satisfied with what we have heard of the advertisements.

Mr. Serjeant Parry. You do not wish to hear the whole of the advertisements?

The Juror. No; nothing more.

Mr. Serjeant Parry. Will you allow me to say my client may be taken somewhat at a disadvantage, to have that put in which is a somewhat fragmentary advertisement. The publisher has been summoned to appear with all the papers he has published from the beginning.

The Lord Chief Justice. This will show the style; one advertisement will do as well as the other.

Mr. Serjeant Parry. No doubt.

The Lord Chief Justice—[Reading extracts from the advertisements]. And so the symptoms are enumerated which should excite apprehension and lead to an examination of the lungs. Here is a long extract as to the sensations which may lead a person to think that his lungs are affected:—"On the Symptoms of Consumption. I have said that in the early stage the most common of these symptoms of consumption are dry hacking cough, a sense of shortness of breath on exertion, and increased frequency of the pulse. Another symptom which should lead us to suspect the health of the lung, is pain. During the course of the disease it is usually present in some degree, but varies very much in intensity. It may be the first indication of tubercles in the lungs, or not appear until after ulceration has taken place. In one case, it is a sharp stitch in the side; in another, a dull aching under the breast-bone; in a third, a sense of burning; while in a fourth, we have only a feeling of weight and oppression. Its seat, too, is often distant from the part of the lung affected. It may be on the opposite side of the chest, or low down in the side, whereas tubercles are always deposited in the top of the lung. Sometimes we find the pain immediately over the part affected, but this is not usual. The reason for the vagrancy of this symptom will be understood by bearing in mind that the pain is not actually in the lungs, but in the walls of the chest, or in the pleura lining them. It is at best but a mere sympathetic vivication, and as such may be caused by disease remotely situated from the point at which it is manifested; just as disease in the stomach frequently causes pain in the head. At times the pain has very much of a rheumatic character; and at others resembles neuralgia. From this it will be understood that pain is a very unreliable symptom. It is no proof of the soundness of the lungs that the

patient has never experienced any pain, since more than one-third of those labouring under this disease never have the least indication of it. The lungs, as a rule, do not manifest injury by pain; even acute inflammation is very rarely attended by any suffering. If there is pain, soreness, aching, weight, oppression, however, it should be regarded as a suspicious circumstance, and the lungs immediately 'sounded' to discover its cause. When considerable disease exists in one lung, patients sometimes find it uncomfortable to lie on that side, and the sense of discomfort produced by doing so is the only inconvenience they experience. The losing a little flesh——" All that is as to the symptoms. I do not know that there is anything in this. So far as this advertisement is concerned, there is not that material part with regard to what he said about the cure. Anybody who has had anything to do with consumption has no difficulty in describing what the symptoms are, but this advertisement stops short of the cure, and what he recommends the patients in the way of cure. We have not got them, and shall be glad to see them.

Mr. Serjeant Parry. In point of fact, that is an extract from the book itself?—It is an extract from the book.

Mr. Karlake. I believe it will be found, when the whole of the papers are put in, that they do not follow the letters as published in the book, but to a considerable extent they do.

The Lord Chief Justice. The system adopted by Mr. Mitchell seems to be to give so much of the heading and so much of the extract from the book as would fill up the column.

Mr. Karlake. So that by degrees you get the whole of the book.

The Lord Chief Justice. Taking this step by step, and by instalments, you get the whole.

Mr. Serjeant Parry. There are several chapters in the book itself, and each chapter is headed "Letter;" for instance, "Letter 20. On the Cure of Pulmonary Consumption." Then "Letter 21. On the Cure of Pulmonary Consumption—Continued." And "Letter 22. On the Cure of Pulmonary Consumption—Continued."

The Lord Chief Justice. In each of these chapters there are interspersed letters from persons in the usual way, who have witnessed cures, or who have been cured.

Mr. Serjeant Parry. There are, no doubt.

I will ask you this question so as to explain what these advertisements were. They were extracts from your book, and carried on from day to day in advertisements?—I think probably an extract was published about once a week.

And that advertisement is a type, is it, of the advertisements you published?—It is.

Do you know whether the whole of your book was, from time to time, published as an advertisement?—Well, I presume the principal part of it. I could not answer definitely, because it was very much in the hands of the publisher.

I believe your treatment of consumption has mainly been through inhalation of various drugs vaporized?—The only difference, I believe, between the treatment I have pursued and the treatment which is generally pursued by the profession, has been that I have used, in addition to the usual means employed, remedies by inhalation.

The Lord Chief Justice. That is the distinction in your mode of cure?—It is only in that respect that it differs; it involves all other methods of treatment.

Still that is the characteristic?—The principle of inhalation is.

Mr. Serjeant Parry. And from your experience, have you faith in that treatment?—I have infinitely more faith in it than I have in anything else. I do not think it is absolutely specific, but I think it is efficacious.

The Lord Chief Justice. I do not think you state in your book what the remedy is?—I think your lordship will find it there.

Mr. Karlake. We shall have to go into that, because my impression is that he does not, and that is the impression on other men's minds.

The Lord Chief Justice. I ask the question because I once read the book, and it left that impression.

Mr. Serjeant Parry. Are there particular means indicated by which a medical man could adopt your treatment?—Yes.

The Lord Chief Justice. What is the principal remedy?—The principal remedy on which reliance is placed is the administration of oxygen, oxidizing inhalants.

Mr. Serjeant Parry. If you look at page 106, I think you will find it; but I think you ought to explain what your treatment is, in justice to yourself—The principle upon which I rely for the promotion of the absorption of tubercular matter from the lungs is the increased vitality of the system, produced by the inspiration of oxygen. That, I think, is stated in the book as plainly as can be.

Just see whether this is the right passage. I call my Lord's attention to page 106—That is a portion of the explanation.

These letters of yours were addressed not merely to the medical profession, but to the general public?—They were published in the usual form; they were intended undoubtedly for the general public.

The Lord Chief Justice. As far as the advertisements are concerned, of course.

Mr. Serjeant Parry. Yes; I say they were addressed to the general public, not exclusively to the profession?—Not exclusively to the profession; I am not aware that many medical works are.

You say that the principle upon which you rely is that of inhalation. If you are asked questions as to any particular portions of your book, you will be able to point them out. You have faith in that principle, and have acted upon that?—I have greater faith in that than in anything else.

You do not trust entirely and exclusively in your employment of inhalation?—No treatment would be scientific that did not embrace

all the organs of the body ; that did not provide for the regulation of the other organs.

You do not intend this to be a universal remedy, or an exclusive remedy, or anything of that kind ?

The Lord Chief Justice. A specific ?

Mr. Serjeant Parry. He does not say it is a specific even.

As regards consumption, there is no absolute cure known, is there ?—There is no established method of practice.

The Lord Chief Justice. I understand that you do object to the use of ordinary remedies ?—Not at all.

Your book says : “ In consumption the lungs and the blood require more oxygen than they can, in their present condition, derive from the air ; and if the physician does not supply it, he withholds that for the want of which the patient is dying. And, pray, how does he supply it ? Is it by pouring iron mixtures, cough syrups, or fish oil into the stomach ? ” I understand from that, that you object ?—No, but I object to the entire reliance on those means, and withholding what I regard as more important. Employ those as adjuncts, but not as exclusive means.

Does not that passage mean that it was a mistake to give those remedies through the stomach ?—I think it is only to be viewed in the light of an assistant. Certainly, if we had constipation of the bowels, we should require aperients.

The Lord Chief Justice. We are speaking of consumption.—In consumption the treatment embodies all those materials, and no treatment would be rational that did not ; but for the removal of the condition of the blood on which tubercular matter depends, I rely on oxygen.

If a person with consumption happens to have stoppage of the bowels, you treat his bowels with the proper remedies ?—Undoubtedly. That would have little to do, however, with the removal of tubercles from the lungs, but might be a great hindrance to the recovery of the patient.

Apart from any of those other ailments with which a consumptive person may be afflicted, and so far as the consumption or disease of the lungs itself is concerned, I understand you in this book to deprecate getting to the lungs through the stomach. You say that the true principle of success in the treatment is inhalation ?—I deprecate the reliance on the administration of medicines through the stomach, when I regard the administration of remedies through the lungs as much more efficacious and much more scientific.

Mr. Serjeant Parry. In company with this treatment by inhalation, do you also—did you employ other curative means ?—Undoubtedly ; I believe all means known to the profession.

Sedatives, for example ?—Sedatives.

The Lord Chief Justice. It is said here that the remedy is oxygen in such admixture with nitrogen or atmospheric air as shall best adapt

it to the indications of the case. You possibly could not administer the oxygen in its pure state?—Yes, you could.

Then you do not mix it with atmospheric air?—We do not usually administer it in its pure state, but it could be inhaled.

Would it not be too strong?—I am aware that that is the chemical theory, but it has not been borne out by my experience; and I doubt very much whether the blood takes more than the system requires from pure oxygen. I tested the matter by breathing 400 cubic feet of oxygen at one sitting, and did not experience any ill effect whatever from it. I therefore doubt very much the chemical theory which asserts that oxygen is strong in proportion to its absolute purity. I think it is stronger in combination with nitrogen.

"What is the remedy? It is oxygen in such admixture with nitrogen or atmospheric air as shall best adapt it to the indications of the case." I want to know whether it depends on the proportion in which it is united with atmospheric air. You say not: you say the oxygen will do.—If we admit the principle for the necessity of more oxygen established by dyspnœa or want of breath, which is a mere craving for more oxygen, then each medical man must exercise his own judgment on the knowledge of the disease in fixing the quantity, because in one case the lungs are small, and in another very large, and the amount of stramonium would be large in one case and slight in the other. It would not do to lay down one established rule; that would be empiricism and quackery.

Mr. Serjeant Parry. Have you read the works of English medical men of eminence on the subject of inhalation, as dwelt upon or considered in reference to this disease?—Yes.

The Lord Chief Justice. What are the works you have read on the subject of inhalation:—The more recent works, those that have been published since I have been in London, are Dr. Abbot Smith's, physician to one of the hospitals in London, and Dr. Bedell's.

On the subject of inhalation?—On the subject of inhalation.

Mr. Serjeant Parry. I propose now, with your lordship's permission, to take this course: to place in the hands of this gentleman books of medical science, in which the subject of inhalation is alluded to and commented upon, and in which the practice is more or less recommended.

The Lord Chief Justice. Very well.

Mr. Serjeant Parry. While upon this subject of "inhalation," and belief in its efficacy, will you refer to Dr. Abbot Smith's book? I think it is page 4 or 6.

The Lord Chief Justice—

"Inhalation, like every other remedial means, cannot always succeed; but its great value in the treatment of a large proportion of pulmonary, bronchial, and laryngeal disorders is daily becoming a more generally recognised fact" (p. 6).

that is the passage you mean?

Mr. Serjeant Parry. Yes; and there is another, at page 4:

"The author, in writing the paper on Inhalation (*Medical Mirror*), desired chiefly to draw additional attention to the value of this method of treatment, and to vindicate the medical profession from the charges which have been brought against it, in certain quarters, of ignorance of, or indifference to, its merits" (p. 3).

Mr. Coleridge. And there is one at page 17.

The Lord Chief Justice—

"MM. Demarquay and Lecont, he tells us, found that 'the inhalation of oxygen is soon followed by an improvement in strength and spirits, and often greatly increased appetite. The lips and surface of the body assume a more healthy colour, greater vitality is manifest, and much of the nervous irritability previously present disappears'" (p. 17).

Mr. Serjeant Parry. Those are the only passages.

The Lord Chief Justice. There is a passage I see also at page 18: "Other observers——"

Mr. Serjeant Parry. Now I refer to the *Lancet*, 11th February, 1865. I want to see the article beginning, "The utility of the topical impregnation of the air passages." What is the title of the article?—"New Inventions in aid of the practice of Medicine and Surgery," by Dr. Wilson. The passage is—

"The utility of topical medication of the air-passage, by the inhalation of the vapour of water impregnated with various substances, is extensively recognised by the profession. The absence of any simple and efficient apparatus for the purpose is often the only reason why the great relief which such applications are capable of affording is withheld from the patient."—*Lancet*, Feb. 11, 1865.

Have you adopted an apparatus of your own invention, or of anybody else, which you find successful?—I have used the inhaling instrument which I used in 1852.

Is that an invention of your own, or of anybody else, you have adopted?—Well; it is an invention of my own, modified from time to time until it reached its present shape.

Have you used various drugs in this inhalation practice of yours?—I have.

Various drugs known in the general Pharmacopœia?—They are all known in the Pharmacopœia.

There is no secret or empirical medicine that you use?—No; nor any pretence of it.

And in your treatment by inhalation, I presume the patient inhales from time to time, according to your judgment and discretion, various substances that are vapourized. Is that so?—Undoubtedly; each inhaling mixture is composed of the various ingredients.

Give one or two instances.—Stramonium is a very common sedative added to an oxidizing agent, and belladonna also is a very good addi-

tion, and, in fact, one of the most common. Chloric acid is one of the oxidizing agents which we employ extensively.

That will be sufficient to inform the jury what the nature of your practice is. Now, this is a work of Sir Charles Scudamore, 1834, entitled, "Cases illustrating the remedial power of Inhalation of Iodine in various distempered states of the Lungs and Air Passages," &c., &c. Is that a work that you have read?—I have read it.

Mr. Coleridge. I do not know whether it will be convenient to your Lordship, and I am in the hands of Dr. Hunter; but here is a copy of printed extracts which we could put in, if you please to have them in this shape.

The Lord Chief Justice. Yes.

Mr. Karlake. Let me have a copy.

Mr. Serjeant Parry. My Lord, I put in this book from which the extracts are taken, and subject to that, I will read from the copy, if you will allow me. It is from page 208 of Sir Charles Scudamore's book—

"The diseases in which I consider it proper to adopt the inhaling method are "some kinds of cough; certain asthmatic conditions of the air-passages; chronic "bronchitis; and, above all, tubercular consumption" (p. 208).

Mr. Karlake. That follows his description of what he orders to be inhaled.

Mr. Serjeant Parry. No doubt; I am showing simply the principle of treatment by inhalation is not new. It is not mere quackery :—

"I have been consulted in many cases in which the disease had proceeded to so "great an extent as to preclude any reasonable expectation of success; but I have "thought it my duty to make the attempt. In some of the cases related in this "volume I despaired of success, but had the great satisfaction of obtaining it by "perseverance. It will not be imagined that I have less confidence in the "importance of inhalation because my professional brethren either pay no attention to the practice, or make trial of it in a slight and inadequate manner. "Every one must admit the large occasion there is for leaving the beaten path "in the treatment of consumption, so almost universally does death make its "victims in this disease" (p. 211).

Then, page 113—

"I have in several cases succeeded in producing the absorption of tubercles "by the continued influence of the inhalation of iodine. . . . I also believe "that it does, in the most favourable manner, assist the softening process, when "the disease has come to that stage; causing a more free expulsion of the "tuberculous matter by expectoration; inducing a more healthy condition of the "bronchial mucous membrane; very probably dispersing crude tubercles by the "stimulus given to the absorbents; and finally assisting the healing process "in the ulcerated cavity" (p. 213).—*Sir Charles Scudamore, M.D., F.R.S., &c.*

Do you use Iodine in that disease?—In certain classes of cases.

Do you use conium?—I use conium continually, regarding it as a great sedative.

Do you use tar?—Occasionally; but very rarely.

The Lord Chief Justice. Those remedies you use are not mentioned?—They are all well known to medical men.

But not mentioned?—I mention four classes of remedies that were to be employed for the purpose of inhalation.

The Lord Chief Justice. What were those four?

Mr. Karlake. There are no substances mentioned: “Expectorants, alteratives, sedatives, and astringents.” It does not say what sedatives, and so on.

The Witness. As the work was intended for popular reading, it would scarcely be warranted for me to write out prescriptions for patients to administer to themselves what they thought proper.

The Lord Chief Justice. This is a valuable discovery, which you have given to the medical world?—Every medical man knows what are expectorants as well as I do.

That can be inhaled?—That can be inhaled. There are only certain expectorants that can be inhaled, and he knows this as well as I can tell him; and then as to sedatives, it would be for him to use his judgment as to what form of sedative he might select.

How do you volatilize them? Are they administered in the form of vapour, or what?—They are tinctures generally—saturated tinctures, or simple tinctures inhaled from hot water. The instrument regulates the quantity by its size. The quantity of hot water would, of course, regulate the amount of sedative to be put into it; of course, if the quantity of hot water was increased, the sedative would have to be increased also, as it would be diluted, and less volatile.

Mr. Serjeant Parry. As regards any local application, they can only be applied by means of inhalation to the lungs, is it not so?—The only way in which topical application can be effected is by inhalation, by gas, by vapour, or by injecting the lungs with fluids.

Now, from Sir Alexander Crichton’s book I will read this:—

“Physicians of equal reputation and equally extensive experience have been found at all times, the present day not excepted, whose prescriptions for the consumptive not only have very little medical analogy with each other, but are in fact evidences of very contradictory indications (p. 11). . . . That pulmonary consumption cannot be cured by medicines which act through the medium of the stomach, the whole history of our art proves to us. . . . Judicious applications to an ulcer, whether arising from a vice in the constitution or local injury, are equally necessary for its speedy healing. In external ulcers arising from constitutional causes, such as a scrofulous disposition, no one trusts to internal remedies alone; but in ulcerated lungs in which, for various reasons, local applications are most necessary, they are almost totally neglected. . . . If it be asserted that ulcerated lungs are incurable because no remedies can be kept in contact with them . . . or because the ulcer is at all

"times exposed to the pernicious effects of the common atmosphere with all its variations of temperature and electricity, it may be answered that, under all these disadvantages, cases well authenticated of recovery are on record, and that the medicines which can be brought into immediate contact with the diseased parts are both powerful and numerous; but that we are as yet in the infancy of this art. Our knowledge of the volatilised substances capable of being inhaled and of doing good in pulmonary complaints is still very limited. . . . It is, however, certain that the partial success which has attended the trial of many aëriform as well as volatilised substances which, upon being breathed, have come into contact with the lungs of consumptive patients, has been remarkable, and has animated the discoverers with more than ordinary, perhaps more than reasonable hope; and has given them the appearance of enthusiasts with the more sober-minded part of the profession."—*Sir Alex. Crichton, M.D., F.R.S., &c.* (p. 15.)

Then Dr. Maddock. The title is "Pulmonary Consumption," &c. &c. Do you know what title that gentleman has?—I do not. Probably the degree of a Doctor of Medicine.

It does not say London, it may be of Scotland?—He is son of the late Member of Parliament.

This is the extract from Dr. Maddock's work:—

"Although the reasonableness and importance of inhalation, or the local action of medicated vapours in diseases of the air-passages and lungs, cannot be questioned, it must be admitted that it has not obtained for itself in this country that extent of inquiry and examination which it deserves. . . . And yet it is not easy to imagine how this mode of treating diseases of the breathing organs should have been neglected, its feasibility so self-evident, and in such accordance with the theory, principle, and practice of medical science, and the teachings of common sense; for it is an admitted fact that remedies directly applied to the absorbing surfaces of the lungs, independently of the specific local influence they exert, are carried into the system and produce analogous effects as when directed to the surface of the stomach (p. 2). . . . It may be asserted that, although all the members of the profession approve of the principles on which the system of inhalation is founded, that scarcely one practitioner in five hundred has employed it as a remedial agent; and this strange apathy has been exhibited, it must be remembered, in the treatment of those diseases declared incurable under the old routine practice (p. 4). . . . It is a source of unmingled satisfaction to us to be able confidently to assure the public and the profession that there is now a well-grounded hope of recovery for the afflicted, and that consumption is no longer to be considered beyond the reach of the medical art. . . . We shall incontrovertibly show, not by theoretical speculations, but by facts furnished by the experience of highly talented practitioners, and of ourselves, that pulmonary consumption in certain stages of the disease is positively curable, and that, under the most adverse circumstances, it is possible to afford extraordinary alleviation of suffering by a judicious use of medicated inhalations.

"But while we confidently assert that consumption may be cured, let it not be supposed that we regard inhalation as a catholicon possessed of the power of overcoming the disease in every stage and under all circumstances. We fully admit the formidable character of pulmonary disease, and the utter uselessness, in very many instances, of the best directed efforts to oppose its progress; but surely occasional failure cannot be used as an argument against our mode of treatment, inasmuch as all remedial means so frequently fail in complaints affecting the liver, stomach, womb, kidneys, and other organs, with the proper treatment of which the members of the profession generally are well acquainted" (p. 61).

"The *rationale* of inhalation is exceedingly simple, indeed nothing can be more simple; and it will be at once evident to any person who will give the matter one minute's consideration that this plan of treatment is based upon strictly scientific and correct principles, for it requires no professional learning to perceive that, from the relative position of the stomach and lungs, remedies must necessarily be more effective in diseases of the lungs when introduced into the whole aërial cavity and to the absorbing surfaces of these organs, than when exhibited through the stomach, where they must undergo great and unknown changes from the process of digestion, &c., and can only reach the seat of disease by means of the circulation" (p. 106, edition 1851).—*Alfred Beaumont Maddock, M.D.*

Now, the next book is Dr. Birch's, "On the Therapeutic Action of Oxygen":—

"The writer presumes that sufficient reason has been adduced to show plainly that many circumstances and conditions exist which interfere with the normal and necessary balance of oxygen and food in the organism; that deranged functions and diseases are the result, and that an increased supply of oxygen, therefore, affords a direct and scientific method of applying the needful remedy (p. 28). . . . The preceding observations naturally lead to some considerations, in relation to the therapeutic employment of oxygen in consumption, calculated to make a complete change in the treatment of this Attila—this scourge of the present generation of the human race. The ill-success attending the ordinary treatment of this disease, and the consequently fearful mortality, are so clearly acknowledged, that no apology is needed from me in suggesting the essential cause of this fatality, viz., the negative error of overlooking and entirely neglecting that great element, oxygen, in the management of advanced stages, and the too frequent oversight in securing healthy pulmonary expansion (*i.e.*, the due absorption of atmospheric oxygen), when advising general prophylactic measures in subjects evincing a consumptive tendency (p. 62). . . . Although consumption is occasionally developed in constitutions not in the slightest degree apparently predisposed to it from hereditary idiosyncrasy or from temperament, yet persons of strumous diathesis are so peculiarly marked out for and include such a large majority of its victims, that it cannot but be closely associated with struma and scrofula. As I have spoken confidently of the judicious medicinal use of oxygen as the

"great basis of successful treatment in all cases characterised by this diathesis
 "so I can with equal fearlessness assert that consumption, except in the last
 "stages, is quite amenable to its influence, and claims its best rendered
 "services" (p. 63).

Now, Dr. Carpenter's book "On the Principles of Human Physiology," 1864. The extract that I will read is:—

"That absorption of volatile matters diffused through the air is continually
 "taking place by the lungs, is easily demonstrated. A familiar example is the
 "effect of the inhalation of the vapour of turpentine upon the urinary secretion,
 "It can only be in this manner that those gases act upon the system which
 "have a noxious or poisonous effect when mingled in small quantities in the
 "atmosphere; and it is *most astonishing to witness the extraordinary increase in*
"potency which many substances exhibit when they are brought into relation with
"the blood in the gaseous form" (p. 298).

The Witness. There is another extract from Carpenter.

Mr. Serjeant Parry. The copy that was handed by my learned friend Mr. Coleridge, was not the complete copy of the extract. Allow me to hand your Lordship this one in addition; it is partly written and partly printed. There is a work by Dr. Forbes, we have not the original here. At page 300 of Carpenter's work there is an extract:—

"It cannot be doubted that miasmata and other morbid agents diffused
 "through the atmosphere are *more readily introduced into the system through the*
"pulmonary surface than by any other, and our aim should therefore be directed
"to the discovery of some counteracting agents which can be introduced in the same
"manner. The pulmonary surface" (he continues) "*affords a most advantageous*
"channel for the introduction of certain medicines which can be raised in
"vapour when it is desired to affect the system with them speedily and powerfully."
 —Wm. B. Carpenter, M.D., F.R.S., Registrar University of London, &c. (*Principles of Human Physiology.*)

This is from the *Lancet*, Sept. 23rd, 1865:—

"Great sensation was created when Sales-Girou first presented his portable
 "'pulverisateur' to the Académie de Médecine, and that the most renowned
 "authorities set to work to ascertain by very minute experiments whether the
 "fine spray reaches the lungs or not. It is a well-known fact that these experi-
 "ments answered the question affirmatively and indisputably. Very soon indeed
 "in different countries cases were published which had for a long time resisted
 "treatment by internal remedies, but which had been speedily cured by means
 "of inhalation. It cannot be denied that a new era in the treatment of diseases
 "of the pharynx, larynx, trachea, and the lungs began with the invention of
 "Sales-Girou. Since then the employment of the apparatus has become more
 "general, and it is probable that it will ultimately be in the hands of every
 "medical man and of every patient labouring under a disease to which the treat-
 "ment by inhalation is obviously applicable."—From "*Lancet*," September 23rd.
 1865 (p. 350).

Now, Dr. Reid's book I put in. The date of it is 1844, and the title is "Theory and Practice of Ventilation," &c. &c. The extracts are from pages 216 and 217 :—

"Now, however, that so many gases and vapours have been discovered, and where the movement of the air can be regulated by a ventilating power capable of precise adjustment, it appears abundantly obvious that, were more extensive arrangements made for this purpose, particularly in hospitals, a more extended control might be obtained over the animal economy in many cases of disease, and a series of gaseous remedies brought into powerful operation, through the medium of the lungs, and also by their action on the skin.

"Medical men have long and justly been jealous of any interference with organs so important in their functions and so delicate in their structure as the lungs; but if a proportionate delicacy and care be employed in the remedies applied, certainly no field holds out a more promising path of inquiry than that which is presented in investigating the influence of atmospheric air on the person, and the varied materials which can be mixed with it, and thereby brought to bear more gently and unconsciously on the system, than by any other mode of treatment.

"I certainly am disposed to join with those who consider that the increasing attention now paid to the humoral pathology is well justified by the facts which the progress of science has evolved, and that, in connection with those, there are numerous cases where the more full, the more free, and the more highly sustained action of the air would consume, oxygenate, and burn off from the living frame a large amount of those impurities that are prone to develop or augment disease, exactly in the same manner as air is always tending to oxygenate every external product, where the organic structure, under the influence of the laws of life, does not interpose its peculiar functions, through those wonderful processes of assimilation that can only be conducted in the exquisite chemical laboratories of the living frame.

"What frequent repetition of any ordinary prescription can ever approximate to—

" 20 distinct and separate impulses in . . .	1 minute
" 1,200	1 hour
" 28,800	24 hours?

"and all those acting, not upon a secondary organ—not subject to any intermixture with the food or products of digestion—but conveyed directly to the blood in the lungs, and presented to an area many times exceeding that of the surface of the body.

"By constructing a chamber where the quality of the air that passes the zone of respiration might be entirely under control, and medicated, heated, dried, moistened, cooled, and applied in any quantity, as circumstances might dictate, a more specific power could be obtained, capable of being applied advantageously in numerous cases of disease."

Now Dr. Wilson—this is from the *Lancet*, of 1842. It is a report of

Dr. Wilson's "Lecture on the Principles and Treatment of Pulmonary Consumption :"—

"I believe the good effects of inhalation very materially depend on the taking "in an extra quantity of atmospheric air. The chemical facts connected with "this subject are very striking. An adult man inspires thirty-two ounces of "oxygen daily, which will convert twelve ounces of the carbon of the blood into "carbonic acid; and as the elementary constituents of tuberculous matter are "principally *carbon and hydrogen*, it is not improbable that, had there been a "sufficient quantity of oxygen present at the time of their secretion, they might "have been converted into carbonic acid and water. However, as I am prosecuting this investigation, I shall reserve the facts connected therewith until a "future time, but the effects produced founded on this view are very startling" (*Lancet*, vol. ii., p. 573, 1842).—*W. Wilson, M.D., Physician to the West London Institution for Diseases of the Chest.*

And then there is Dr. Walsh's book, 1860, "A Practical Treatise on Disease of the Lungs," &c. &c. :—

"The inhalation of tar vapour, creasote vapour, iodine or chlorine, most unquestionably reduces the irritability of the mucous membrane and the quantity "of secretion. The results of M. Cottureau with chloruretted inhalations are "peculiarly important, and show, as admitted by Louis, that singular advantages "may be obtained through them, even where the general symptoms closely "simulate those of phthisis. Of the remarkably beneficial effects of creasote "vapour, in particular, I can speak with confidence: the cases are rare where it "fails to agree from the first; but in very irritable constitutions the mucous "membrane may gradually be prepared by the inhalation of extract of hyoscyamus or conium. If there be spasmodic tendency, the latter drug may be "rendered powerfully anti-spasmodic and sedative by the addition, on the "instant of use, of a few drops of liquor potassæ (p. 218). I have no experience "of inhalations of oxygen, hydrogen, or carbonic acid, and but little of moist "inhalations, chloruretted or ioduretted: the latter I have recently found relieve "some forms of phthisical bronchitis. Inhalation of dry iodine vapour certainly "controls excessive secretion from the tubes, and has occasionally appeared to "improve the constitutional state" (p. 518).—*Walter Hayle Walshe, M.D., F.R.C.P., Professor Principles Med. University College, Physician to the Hospital for Consumption.*

Now an extract from Dr. Louis's work, translated by Dr. Cowan. You know Dr. Louis?—Yes.

Who was he?—A distinguished authority.

Upon this disease of consumption?—Yes; one of the greatest authorities.

This is a treatise of Dr. Louis, 1836 :—

"The application of medicated vapours to thoracic affections may be traced "back as far as the writings of Galen, who speaks highly of the vapours from "experiment (p. 378) Their real value is not, however, on this "account diminished, and after an examination of the works of Gannal,

"Murray, Scudamore, and Cottereau, we do not hesitate to say that the evidence "in favour of the palliative effects of chlorine and iodine in consumption is "amply sufficient to encourage others in the application of these remedies. "The cases published by Cottereau are by far the most satisfactory. We would "refer to the first also published by Gannal, and to the twelfth, as particularly "striking and decisive as to the existence of pulmonary tubercles. Sir C. "Scudamore insists strongly on the power of iodine in facilitating expectoration "diminishing expectoration, and promoting sleep and appetite. Dr. Thompson "in his "*Materia Medica*," speaks very favourably of the palliative action of "chlorine in consumption; all his trials were upon advanced cases "It invariably gave relief, and, as he expresses himself, may be said to have "scattered flowers on the borders of the grave" (p. 379).—*Louis, Translation by Dr. Cowan.*

Then Sir James Clarke—"Treatise on Pulmonary Consumption and Scrofulous Diseases, by Sir James Clarke, M.D., 1835 :—

"The inhalation of volatilised substances in the form of dry fumes and of "watery vapours has been supposed to be beneficial in consumption from their "being applied directly to the seat of the disease (p. 366). Before we can "decide on the particular cases to which they are applicable, we would require a "series of experiments conducted by practitioners well acquainted with the "nature of pulmonary disease" (p. 367).—*Sir James Clark, M.D., F.R.S., Physician in Ordinary to the Queen, &c.*

Then I have a final extract from Dr. Evan Riadore, "On the Remedial Influence of Oxygen, by James E. Riadore, 1853," an English physician. This is at page 55 :—

"I have found that persons, after being at parties and balls, have been quickly "relieved of their dry skin, headache, and lowness of spirits, by inhaling oxygen "a few times; indeed, I have often been told by my patients that they never had "a gentle perspiration over their bodies except after inhaling the oxygen, which "always insured a cheerful state of mind, instead of that miserable feeling with "which they had been previously afflicted. When defective oxygenation exists, "the various preparations of iron should be given to increase the amount of "red particles in the blood, and thus in the capillaries influence the quantity of "oxygen which is absorbed. I have also found that, in the serous temperament, "the benign influence of inhaling oxygen is increased by the patient at the same "time taking iron. In lax and debilitated cases, this mode of treatment is "highly to be commended. Absorption of oxygen by the skin is another "pleasant mode of administration. Gases act upon the constitution when "absorbed as substances do when mixed in water or in steam-baths."

Mr. Karlake. But you will not find that this follows; if so represented, it is erroneous :—

"I find that persons, after being at balls and parties, have, by inhaling oxygen "a few times, been relieved of low spirits"—
and then it stops.

Mr. Serjeant Parry. It does not immediately follow:—

“Those absorbed, when mixed in water or in steam-baths, either upon the nerves or upon the blood; on the nerves as stimulant or sedative; and they remove from the blood extraneous substances which may have been absorbed into it.”

Mr. Karlake. I think, in order to understand that extract, we ought to begin at the beginning:—

“When defective oxygenation exists, the various preparations of iron should be given to increase the amount of red particles in the blood, and thus in the capillaries influence the quantity of oxygen which is absorbed.”

He is not talking of inhalation:—

“I have also found that, in the serous temperament, the benign influence of inhaling the oxygen is increased.”

Mr. Serjeant Parry. You see, my Lord, we cannot read the whole of the work; and I do not know that these extracts have been taken with any unfairness at all.

The Lord Chief Justice. Still, Mr. Karlake means where the context would modify it. It is well, perhaps, to have it at the moment.

Mr. Serjeant Parry. Yes, certainly. I am reading now from page 63:—

“The treatment of diseases by oxygen gas, and other factitious airs and medicated baths, is but beginning to excite attention.”

Then I go to page 86:—

“Although the proportion of oxygen in the atmosphere is, doubtless, the best for average health, still its volume must vary with the temperature of the inhaled air, consequently medical men have found that, in certain diseases, an increase in oxygen gas over the natural proportion in which it is found in the atmosphere is exceedingly beneficial; because oxygen, in warm close rooms, as I have already pointed out, is continually found to be insufficient to preserve health; whilst carbonic acid, under similar circumstances, is found proportionably too abundant. The latter circumstance produces headache, dry hot skin, and lowness of spirits.”

Now page 91:—

“Doubtless all inhaled remedies applied immediately to the diseased lung and to the blood of the patient must be more effectual than when swallowed, digested, diluted, and circulated in the blood vessels,” &c.

Page 6-7:—

“Doubtless some cases of Consumption are incurable, but in the most desperate circumstances I can attest that more or less relief may be afforded by some of the means to which I have referred; and it is surely right to give every

“invalid the chance of receiving benefit rather than leaving him to his fate, or
“send him to another climate only to find a foreign grave.

“Whatever may be the *à priori* opinion with regard to these remedies, I may
“state that in no instance have they, in the various diseases in which I have
“employed them, proved injurious; nevertheless my confidence in, and recom-
“mendation of, these old remedies may be totally disregarded by the sceptical
“portion of the public, and may possibly disappoint that portion who more
“readily confide in that which is new than in that which is true. Scepticism
“and credulity are twin monsters who equally oppose the ever simple and
“smooth course of experience on the current of truth.”

I believe I am right in stating that you have studied the works from which these extracts have been taken?—I have read the works.

I think you have explained your mode of treatment. You told me just now that you employed various drugs from time to time. I believe the druggist with whom you dealt for these drugs is present?—Yes.

And that you employed no secret or unknown agent which you have not told the world about?—No.

And you do not pretend anything of the kind?—I do not pretend anything of the kind. I have no secret from members of the profession. I do not think it desirable to enter into details with non-professional people.

You have no remedy which you keep to yourself which you employ. You act on purely scientific principles, with drugs and remedies which are known to the world?—I believe that is so.

You practised in 1864, and so I must ask you now, had you amongst your patients a Mrs. Merrick?—I had.

Now, about what time was it that she became a patient of yours?—In October, I think.

The Lord Chief Justice. What year, 1864?—It was either the latter part of September or beginning of October 1865.

Mr. Serjeant Parry. She became a patient of yours?—Yes.

How long did she remain a patient?—She was a patient about five weeks.

I believe—we know, in fact, that she made a charge of rape against you?—Yes.

Of which you were acquitted?—Yes.

You may as well, as you are in the box, state upon oath—as I know you will state—that there was not a word of truth in that accusation?—Not a word of truth. I may as well state also that the woman was not at my house even at the time she alleged.

I must not go into the details. You were acquitted by the Jury, and you say now, on your oath, that there was not a word of truth in that accusation?—Not a word of truth.

Before that accusation was brought against you, had you made a charge of assault against the husband of that woman?—I had.

And her brother, I think?—And her brother.

Was that charge pending when the charge against you was made?—It was.

Now, after you were acquitted of the charge upon which you were tried, did you prosecute the charge of assault against the husband and brother of that woman?—I did.

And were they convicted?—They were convicted.

And did you present yourself then for examination and cross-examination in the witness box?—I was placed in the witness box, and examined and cross-examined.

I believe her husband was sentenced to two months' imprisonment, and a lesser sentence was passed upon the brother?—No, the reverse of that.

The brother two months and the husband a lesser sentence?—Yes.

What was the sentence?—I think he was fined 5*l.*, but it was not till some time afterwards, and I rely upon the newspaper report.

The sentence did not take place?—The sentence was deferred at the time of the trial.

Now, something has been said about enormous fees as a matter of comment on this libel. Allow me to ask you what have been your fees?—The usual fee of a guinea for consultation, and an examination of the chest, with an opinion written or otherwise.

And that has been the mode of your procedure—first, to examine the patient and make a diagnosis of the complaint. Is that so?—That is the invariable course.

A guinea, you say, to begin with. Supposing they continued your patients, what has been your usual charge?—The fee is 5 guineas for a month's treatment, inclusive of medicines and attendance, that is, consultations as often as they are required. As the diseases are chronic and require continuous attendance, and very often frequent visits, that rule is established.

You could not charge them anything every time you saw them. You would sometimes see them every day in the month?—Sometimes they would require to do so; sometimes three times a week, and the fees and the ordinary fee would be more than they could pay.

What were your fees in America?—They were similar; I think the same rule has been adhered to throughout the whole of the practice; there has been no material difference.

Now, did that fee of 5 guineas include all drugs that were administered?—Yes, and an inhaling instrument.

Did they always inhale at your place?—They very seldom inhaled at my place.

Did they take the instrument home with them?—Yes.

And used it according to your prescription—Yes.

Were you in the habit of visiting occasionally?—I visited occasionally.

Did Dr. Melville and Dr. McGregor, the gentlemen associated with you, visit?—Yes.

And Dr. Croser?—He was an assistant.

I believe he is a medical practitioner, practising in Liverpool now?—Yes. They are all registered medical men.

I believe at times, when the persons were not in a condition to pay the whole of your fee, they were reduced?—The fees were reduced very frequently, and very much sometimes below the actual cost, where circumstances required it.

Where the conditions of the patients were such as not to allow them to pay the whole of it?—Yes; and in fact we also had a large number of charity patients.

Gratuitous patients?—Yes.

This libel was published the 10th of November?—Yes, the 10th November.

At that time we know from the dates that the charge of rape was pending against you?—The investigation was to take place on the 12th.

On the 7th of November you appear to have been charged; you mean that the hearing was postponed to allow of evidence for the defence to be got up?—Yes.

The libel that we complain of here was published on the 10th November?—Yes.

Did you appear at the Police Court after that?—On the 12th.

Was that an adjournment from the previous day?—It was an adjournment.

From the 6th, I think?—From the 6th or 7th.

And that fact was stated in all the newspapers?—Yes.

So that this writer in the *Pall Mall Gazette* must have known that it was an adjourned case?—Yes.

What was the date of your trial?—I think it was the 21st, but I really could not charge my mind with the exact date. The papers I have.

I will take it as the 24th?

Mr. Serjeant Parry. I do not know whether your lordship will allow me to put this question. It would come better, perhaps, at a different stage, but will not come very inopportunistically now. It is this: whether, in the letters which he received from Sir George Grey and Dr. Burrowes he was addressed "Dr. Hunter."

Mr. Karlake. I must object. I know nothing about Dr. Burrowes or Sir George Grey.

The Lord Chief Justice. Probably the writer of that paper never saw the correspondence between him and Sir George Grey and Dr. Burrowes.

Mr. Serjeant Parry. That might be presumed, although what the writer saw we have no earthly means of knowing.

The Witness. The letter to Sir George Grey was published.

Mr. Serjeant Parry. You did write, as a matter of fact, as you have told us, to these officials. Was the correspondence subsequently published in the *Lancet*?—The first letter.

Mr. Karlake. I must object to this. It is intended to produce some effect or other, like my learned friend's opening speech.

Mr. Serjeant Parry. Very well.

Are you able to say whether your practice fell off after the publication of this libel?—Oh, undoubtedly; very decidedly.

Can you say in what proportion, or how much of it, or what?—I should say fully half of the practice, certainly.

Cross-examined by MR. KARSLAKE.

As my friend thinks it wise to ask that, I suppose your practice fell off after the charge of Mrs. Merrick; was that a fact?—The two facts occurred at the same time; say within three or four days: so, of course, it would be very difficult to separate one from the other.

I see from your statement that it must have been at the age of twenty that you received this medical degree at New York?—Well, it was two months, I believe, after I was twenty-one.

I understood that in '26 you went to Canada when you were about six weeks old, and it was in 1846 the degree was conferred?—I do not know whether it was at the end of 1825 or the beginning of 1826 that I went to Canada; it was merely a general understanding that I have. It is impossible for me, at six weeks of age, to remember all the particulars.

When was it you first began to devote yourself to the treatment of pulmonary complaints?—About 1851.

Had you at that time read the works which you quote in your advertisements?—No, I had read none of them.

Then was it an invention of your own, or a discovery?—An invention? I could not apply that term; it was a conviction that remedies might be employed in that manner, and might act on the lungs, from a knowledge that I had of the views of Professor Carpenter on the subject, which I had read.

What other works had been published at that time in which inhalation was mentioned as the means of getting rid of the painful effects of consumption or bronchial disease?—Well, there had been no works that had fallen under my notice, except observations from running through many works, and a paragraph here and there, but they have attracted my attention.

They had not attracted your attention at that time?—They had not attracted my attention before I began to study the subject myself. After that, I had referred to all the works on the subject.

Then you found that inhalation had been recommended and practised previously?—Undoubtedly.

I did not quite follow what your course was after that. I understand

in 1851 you had an attack of spitting of blood, and were obliged to give up practice?—Yes.

When was it that you took to practising for pulmonary complaints and bronchial complaints?—I can easily answer that question by stating what course I pursued. I went to New York and consulted the leading physicians there, and, getting very little satisfaction with reference to the treatment of my case, further than the advice to go to the south, I decided to go to the south; but before doing so I determined to make use of some investigation with reference to the action of these inhalants on my health. I took the rude contrivance which I prepared at that time, with me, and began to practise on myself. Probably four, five, or six months elapsed before I returned to Canada; and for those four or five months before I returned home, I was experimenting on myself, and I also met with other invalids suffering from pulmonary diseases, who, attracted by the novelty of the remedy, and the fact that I had got benefit from it, induced me to prescribe for them, which I did at first as a friendly act, and subsequently I consented to treat many of those cases, receiving fees for so doing; and, finally, after returning to Canada, fearing to become too actively engaged in the duties of the medical profession, I determined to apply myself to the treatment of pulmonary disease.

What I ask is the date?—That would be in 1851 or 1852.

How long have you continued to practise in Canada?—After that?

Yes; from 1852 to what date?—Well, I left Canada almost immediately.

Then you had hardly established your system in Canada before you left?—I had not attempted to establish it.

When did you go to New York?—About the same time in 1852.

And practised there?—I did not settle in New York. I went to New York, and remained there a short time visiting the hospitals, and, not being strong in health, I went from there to Albany, and attended the medical college at Albany, and thought of engaging in teaching as a private teacher. From Albany I went to Washington, and there I established myself and remained a year.

Were you applying yourself there to this complaint?—I remained there with my family, and settled for the purpose of practising my profession.

For the cure of pulmonary complaints or the profession generally?—I had, before I abandoned general practice, the belief that I had not the physical constitution to bear it.

Did you hold yourself out as a general practitioner, at Washington, or as a person who had studied particularly pulmonary complaints, and who was to be consulted on those complaints?—Entirely.

In what year did you go there, and how long did you remain?—I remained there a year; till some time in 1852 or 1853. I think it was some time in 1853.

In 1853; where did you go then?—To New York.

How long did you remain in New York?—I was continuously in New York, without ever leaving there a day, until the time I came to England, and that would be in 1857, as near as I can recollect.

How long were you absent from New York before you resumed practice again in the States?—I was absent again three or four months.

And then you went back again to New York?—Yes.

And how long did you practise in New York before you went to Canada?—I went to Canada in the following summer.

And how long did you remain there?—About eighteen months.

And then, having returned to New York and resumed there, how long did you remain?—Well, the practice was continued up to the time I came to England; but I went over to Canada a year before. My brother remained at New York, and the practice was continued.

By whom?—By my brother and an assistant medical man.

Who was that?—Dr. Gartland.

Did Dr. Macgregor ever attend there?—No; he was never in America, I think.

Nor any of the gentlemen who are now associated with you?—Yes; Dr. Melville was, I think, with me all the time in New York.

Were these letters composed by you in America?—These letters were composed by me.

At what date was it that you composed these letters?—It was at different periods, but my book was composed at New York in 1853, and my book embodies the substance of these letters.

Was it your practice, while you were resident at New York, to publish the letters by way of advertisements in the newspapers?—In the first instance, after the publication of the book, the substance of these letters was addressed to newspapers, and published in the reading columns, the same as medical men publish letters on the cholera and cattle plague.

Were they published in the *New York Tribune*?—Yes.

In the *New York World*?—The *World* was not established at that time.

Have they been published in the *New York World*?—Yes.

The *Detroit Free Press*?—*Detroit Free Press*.

New York Herald?—Yes.

The *Wilmington Journal*?—I could not say that; but very likely.

The *Lynchburg Republican*?—I cannot say that.

If you will look at your book, and the letters at the end of your book, I think you will find you have?—I think it is very likely, but if you ask me to give a definite answer, it is impossible for me to do so, because I do not charge my memory whether it was published in that particular paper or not. I think it is very likely.

Was it published in the *Wilmington Gazette*?—That I do not remember.

The *Louisville Journal*?—Very likely.

The *Pittsburg Despatch*?—Yes.

The *London Canada West Free Press*?—Yes.

In all these papers they were published periodically.—They were at different times.

And in several other papers there?—And in several other papers there.

I will take it that during the time you were in the United States you kept up a system of advertising these letters in the papers?—I believe that keeping up a system of discussion with the view of propagating my particular views on that subject was what I aimed at.

And the effect of propagating your views was to bring patients to you?—I believe it was both beneficial to me, and decidedly beneficial to the patients themselves.

Besides these letters of yours, were the letters of the grateful patients from time to time that were written to you also published?—No; but the letters of patients were published by themselves.

Without your knowledge?—Many of them.

Some with your knowledge?—Probably some; if they expressed their desire or intention of doing so, I am sure I should not have offered the least obstruction, but should rather have encouraged it than otherwise.

However, I take it that some of the letters appearing in your work were letters which found their way into the newspapers?—I believe that every letter quoted from a publication found its way into that publication, and appeared as quoted in the book.

Now then, having left Canada and come to England, you say that you put yourself in communication with Sir George Grey and Dr. Burrows. Was it after you had been in communication with them that you set up in Seymour-street?—No.

It was before, was it?—It was before.

Then you knew of the difficulty of appending M.D. to your name?—I did after I arrived; not as appending M.D. to my name, because that is perfectly usual. I saw the name of Dr. Sims, for instance, appearing in the *Lancet* every day, and his book advertised in the public papers, and therefore I did not think it at all an unusual thing.

Did you find it afterwards unusual?—No, I do not think it is unusual now. I think Dr. Sims' book is advertised at the present time.

Do you know that it is penal by Act of Parliament to publish yourself as M.D., when in fact you are only M.D. of a foreign college?—No, I am not aware of that.

How soon after you came to England did you give instructions to your advertising agent to put these advertisements in the papers?—Immediately upon deciding to establish myself in England I put the book in his hands for publication, and after it was published I directed him to draw up an advertisement, which he did draw up.

Was that after the fifth edition, price half-a-crown came out? It

must have been though, because it was advertised in this very work.—That is a cheap edition.

That is an edition of “Dr. Hunter’s Letters; on Diseases of the Throat, Larynx, and Lungs; their Nature, Causes, and Cure,” price half-a-crown?—I am not aware that there is any objection to reducing the price by binding the book more cheaply.

No; I ask you was it after that that the advertisements commenced?—Not that particular edition, because that is the fifth edition.

Was the fifth edition actually in existence at the time of the advertisements commencing?—It was the first edition.

I ask you whether the fifth edition had been published when the advertisement commenced?—In what?

In the papers?—I am really at a loss to understand the question.

Have there been five editions?—There have.

Was it after the fifth edition had been published?—I have stated that I began to advertise when the first edition was published.

In the *Times* in England?—Yes, in the *Times* in England.

You began to advertise the first edition?—Undoubtedly.

When did you first begin to advertise the first edition?—The first edition of this book was published in September, 1864.

Was that in the same form as it had been published in New York?—No, not in the same form precisely. It was published in chapters in New York, and it embodied a good deal of matter which I did not think was of much value, and which therefore was struck out; but it was published in the form of letters subsequently in America, and the same form was carried out here.

Now can you tell me at all how often the advertisement appeared in the *Times*?—I could not tell you anything about it, I am sure, but you can easily estimate the number of columns.

I ask you to do so.—I really do not know anything on the subject; I paid very little attention to matters of that kind.

Who attended to those matters of detail—those gentlemen associated with you?—My publisher exclusively.

And Dr. Melville?—I do not think Dr. Melville has given any attention to it whatever.

Nor Dr. Macgregor.—No. They have looked over the matters and made suggestions perhaps, but as to giving distinct orders I do not think they have done so.

Now let me just ask you this: were the advertisements published in the *Times*, *Standard*, *Morning Star*, *Record*, *Morning Post*, *City Press*, the *Staffordshire Chronicle*, the *Kidderminster Telegraph*, the *Wolverhampton Journal*, and the *Walsall Guardian*?—I really could not tell you that. I know that it was published in many of them; but as to the *Walsall Guardian* I do not know such a paper; nor do I know several of the others that you have mentioned; but it is possible.

The Lord Chief Justice. Was it published in the same way, with

long extracts.—No. I do not presume so; but it is possible that my publisher may have published a short advertisement of the book. I cannot tell how widely he has extended it, for I am not in the habit of looking at the items of his charge.

Mr. Karlake. Have you looked sufficiently carefully at them to tell me what you have paid for advertising?—No.

Not even within a few hundreds of pounds?—No; not even that.

Can you tell me within a thousand?—Certainly. I may have paid for the publication of the book a thousand pounds.

And for the advertisements?—Well, they all ran in together.

The Lord Chief Justice. If it went through all these editions did it not pay for itself?—No; not charging it at half-a-crown.

It depends on how many numbers were struck off.—You can see that it is a very large book for half-a-crown, and it is impossible that it should be remunerative, if it were extensively advertised, by the sale. A medical book is very seldom other than an expense to the author. I do not believe that one in a thousand of those that are published is other than a direct expense to the author. It is a matter of reputation, but not a matter of profit as a commercial speculation.

Except for the results that it may produce?—The reputation that he makes, if there is reputation, would result, I presume, in practice.

Mr. Karlake. Do you really mean to tell me that you cannot give me a notion within a few hundred pounds of what your expenses have been.—I could not pretend to give you more than a mere guess, because he brings his accounts to me at stated periods, and I give him a cheque for the amount, and I assure you that I never think more about it.

You cannot tell me within a hundred pounds?—I cannot tell you; but I presume he will be able to give you all the information that may be required. It would be a mere guess on my part if I were to attempt to tell you.

The Lord Chief Justice. You cannot tell us within a few hundred pounds?—It runs through a period of two years. All I could do would be to form a general guess of the amount.

How much should you say it would be?—It would be perhaps a thousand, six or eight hundred, or a thousand pounds, but I could do no more than form a general estimate, and then I do not know how much of that was credited to me again for the sale of the book.

Mr. Karlake. Will you undertake to say that for the advertisements you put in the *Times*, and other papers, you have not paid several thousand pounds?—In the *Times*?

Yes.—I am not aware of it. I am afraid that my publisher has lost money if it be so.

Can you give me a notion of what it costs to publish an advertisement of a whole column in the *Times*?—Yes; I think it is twenty-five pounds.

Probably you can tell me this: have your advertisements been put in as "communicated?"—Undoubtedly.

And paid for as advertisements?—That I presume; but probably at a reduced rate; I do not know the exact amount.

That is all left to the advertising agent?—Entirely.

Now, I can see some works which are not bound in the same coloured paper. There is Dr. Macgregor's "Letters on Diseases of the Throat, Larynx, and Lungs," and Dr. Will's on "Pulmonary Diseases," both of which are also published at half-a-crown.—Yes.

Do you know them?—Very well.

Are they published by the two gentlemen in association with you?—They are.

The Dr. Macgregor who publishes the letters is the Dr. Macgregor who writes two prefaces to your editions of this work?—The same.

Now, in this book, the fifth edition, that my friend handed up, the preface is written by Henry Melville, M.D., 14, Upper Seymour-street, Portman-square?—Yes.

Has he published anything on this subject?—I think he was a contributor to the *Lancet*, but I really do not know of what. He has also written many articles on the subject in another medical paper.

J. J. Macgregor, M.D., is the Dr. Macgregor who publishes letters on the throat, larynx, and lungs?—Yes.

Have these been advertised to your knowledge to a considerable extent?—Yes.

By the same advertising agents as those who have advertised your book?—Undoubtedly.

Did Dr. Macgregor assist you in the composition of these letters?—He did not.

Did you assist him in the composition of his letters?—I may have given him a suggestion here and there.

Just look at the language there and tell me if it is not suggested by you, and if it is not your language (handing a book to the witness). Do not take that page, but in that work from 14, Upper Seymour-street, on the throat, larynx, and lungs.—I think the language and style is as different as two styles can possibly be.

Did you assist in forming that style?—No. The facts of my book undoubtedly Dr. Macgregor copied very closely. From his connexion with me he became identified with the practice and convinced of the efficacy of the treatment. As to the description of the disease, he complimented me upon many occasions upon the very admirable description as he has termed it, and as he had my fullest consent to make the largest possible use of my writings, he did make use of them. But as to the styles of the two, they are as different as anything can be.

I will not ask you which was the best style, but did he practice your system, as you call it, in Ireland?—He did.

Did you see the advertisement of his book in the Irish papers?—Undoubtedly.

When did he go to Ireland? because I see two prefaces here, one ending with these words:—"We only trust that the invaluable suggestions so clearly enunciated by the author may lead, by their adoption, to a new era in the treatment of pulmonary disease, and that the night of gloomy despair"—this is the style, I suppose?—Yes.

"And that the night of gloomy despair which has so long brooded over their treatment may yet be dispelled by the cheering and life-giving rays of Hope. J. J. Macgregor, M.D., 14, Upper Seymour-street, Portman-square, London."—Yes.

Was that before he set up in business in Ireland? This is October 24th, 1864.—It was just at the time—oh, no, it was a year before he went to Ireland.

He could not have gone until 1865?—He did not go until 1865.

You think he did not go until 1865?—Just a year ago.

Then Dr. John James Macgregor, M.D., M.R.C.S.E., wrote a preface to the first edition?—Yes.

And that was published in England?—Yes.

It was not the first edition published, because it was published substantially in New York?—Substantially, but it was a different book. The facts embodied precisely the same matter.

Have you a copy of the work as it was published in New York?—No; I had but one copy, and unfortunately I have mislaid that.

August, 1864, I see was the date of the preface to the first edition which Dr. Macgregor produced?—I think about September or October, 1865, he went to Dublin.

It was, I suppose, upon his going that the fifth edition came out, with a preface by Dr. Melville?—It was upon that occasion, or I think it was subsequently. It was on the publication of a new edition.

Dr. Melville also dates from 14, Upper Seymour-street. Portman-square, London, November, 1865?—Yes.

Will you just turn to the last page of that work. "A card, Dr. Melville devotes his entire attention, during his sojourn in England." How long was it contemplated to last at that time?—When I first came I was contemplating eighteen months or two years; but now I think the time very indefinite.

I see that your book has a similar card—"Dr. Robert Hunter, author of the foregoing Letters, will, during his sojourn in England, give his personal attention and supervision to the treatment of all forms of bronchial and pulmonary disease. Hours, 10 to 4 o'clock. 14, Upper Seymour-street, Portman-square, London." Well, then, in Dr. Macgregor's Letters there is a similar card, which I suppose you know of—"Dr. Macgregor devotes his exclusive attention to consultations, &c., at Dublin"?—Yes.

That is where he established business after leaving 14, Upper

Seymour-street, Portman-square?—Yes; that is where he established his practice.

Where does Dr. Macgregor carry on business?—Carry on business! I think the term is rather——

Where is he consulted?—In Dublin, at that address.

Now, the requirements even as early as September or October, 1864, were so great that we find, I think, the practice in London requires the services of no less than four medical men. You will see this in a sort of letter that you yourself write, a sort of advertisement—a sort of preface to the Letters, at page 43, in the Roman letters:—“Within a few weeks a physician enjoying a very large general practice, and who has for thirty years held an important medical appointment, has become so zealous a convert to my views, that he has resigned both to associate himself directly with me in the London practice.” Who was that gentleman?—Dr. George Munns.

Then in the paragraph above that, I see this:—“One medical man, convinced by what he had seen of my practice in London, has established himself at Dublin for the purpose of devoting himself to the special treatment of pulmonary diseases.” I suppose that is Dr. Macgregor?—Yes.

And another, “An army surgeon of great ability and high attainments, has resigned his commission, to establish himself in the same specialty at Edinburgh.” Who is that?—That was Dr. Wills.

The author of this other work?—Yes.

The work at half-a-crown, which I suppose is mainly derived from your letters upon Consumption?—The description of the disease is something that cannot be derived from any particular source. It must be so far description, if accurate, and must be confined. It may be derived from any source. The style of the book and the ideas Dr. Wills very fully adopted; and I think it is very probable that he adopted my language as far as he thought fit.

Have you the least doubt of it?—I have not. He certainly expressed himself in very flattering terms of it, and he had my full consent to do so; and, although he held quite enough English degrees to be able to write a book for himself, still if he copied mine, I consider it a compliment.

It appears that he copied it so thoroughly, that at the end there is a card, “Dr. Wills?”—Yes.

He resigned his commission in the army to establish himself in a special practice, and those are the usual hours of a special practice.

Yes, four gentlemen—Dr. Hunter, Dr. Macgregor, Dr. Wills, and Dr. Melville—each practising in the same profession, and each having the same hours. Did you see that largely advertised in the Scotch papers?—Well; it was advertised.

The Lord Chief Justice. Was there any partnership or joint interest?—No; there was no joint interest. It was an adoption of the views and practice.

Mr. Karlake. Who was the advertising agent in those papers?—The same.

As yours?—And the same publisher.

And was Dr. Wills practising at Seymour-street before he went to Edinburgh?—No; he was not.

He used to be there from time to time?—He was a guest of mine from time to time, and he was there some few weeks investigating the practice after he resigned his commission.

Has he left Edinburgh now?—He has returned to the army.

He has returned to the army, and given up pulmonary diseases from ten to four?—He has given up pulmonary diseases from ten to four.

Was the preface to the fifth edition, with a statement of English cases, written by Dr. Melville at 14, Upper Seymour-street?—It was.

And, of course, published as a preface to your work with your sanction?—Undoubtedly.

Now, was this first chapter, which is signed in the work, "Robert Hunter, M.D., 14, Upper Seymour-street, Portman-square," re-written for the London work? Look at it; "Progress of the vapour treatment in this country."—I do not catch the question.

It is immediately before the contents—it is a sort of preface by you, signed "Robert Hunter, M.D."

Mr. Coleridge. It is No. 36 in roman numbers.—Was that what?

Mr. Karlake. Was it written for the express purpose of being added to this edition, or had that been substantially printed before in America?—It was written for this number.

Entirely for that number?—I think so.

There is a good deal that must have been printed before in America?—The argument is, I think, very likely an adaptation from some of my former writings; but I do not know that. I do not state it; I merely state that I cannot say whether I adopted any former argument to the construction of that particular preface or not. I do not think it at all improbable. If I liked the argument, I should do so.

At the time you left America and established yourself here, had you got any peculiar invention of your own as an inhaling vessel?—Yes; I had a peculiar inhaling instrument.

Had there been a considerable difficulty in the adaptation of any instrument to inhaling before that?—I do not know that there had been any peculiar labour. I was quite astonished to find indeed from a paragraph in the *Lancet* that it was considered so meritorious a thing to devise an inhaling instrument.

We have seen the paragraph in the *Lancet* which does refer to an instrument which was invented after you came to London?—I do not know whether it was so invented.

You told me that when you came there were some rough appliances that you operated upon yourself?—You are going back fifteen years.

I ask you when you came to England had you matured some par-

ticular inhaling instrument?—I had done that twelve or thirteen years before.

You use also Rammadge's inhaling instrument?—No, I do not use it.

It is a modification, is it?—No.

Is it not an adaptation of it?—No.

Is it something peculiar to yourself and your practice?—I do not know that it has become peculiar, for I think they are sold by all the chemists.

You sell them?—No; they are manufactured by the manufacturers of surgical instruments.

I see that the preface of this fifth edition, by Henry Melville, gives a number of cases from England?—It gives a few cases.

Your own writings give the cases from America and Canada. Now do I understand you correctly to say that your practice, as pointed out by this work, is merely the old practice of medical men, relying more strongly on inhalation than they did? Does that correctly describe what you represent to be the object of this work?—The object of that work is to popularize a knowledge of the most prevalent and the most fatal class of diseases known to humanity, and at the same time to explain to the public what I regard as a more successful treatment than any hitherto employed.

Do you use in your practice all the old treatment which is used by medical men?—Most of it.

As you have told us to-day, you claim nothing new in claiming inhalation?—I claim nothing new beyond a greater experience in the administration of remedies by inhalation probably, and the use of a particular remedy—oxygen.

The Lord Chief Justice. Was oxygen never used before?—Undoubtedly; but I was not aware of it at the time I first devoted myself to the study of these diseases.

Mr. Karlake. I am talking of 1864, when you sent forth these letters to the public, were you using the old means which the medical men in this country used, and the inhalation used before, and the different substances which had been used for making vapour?—In some respects they were different.

In what respects?—For example, I do not think that chloric acid was ever used as an oxydizing agent before. I looked over the little manual published by the Hospital for Consumption, and I do not find that stramonium is recommended in their formula, whereas I use these remedies extensively, and rely upon their oxydizing effects. The reason of publishing the work and impressing it on the public was a firm belief in the carbon theory of consumption and in the efficacy of the oxygen treatment. I considered them of very much greater importance than the public or the profession had any idea of. Inhalation in this sense was not generally practised by medical men, and I had the best reasons for knowing it was not understood by medical men.

Am I right in assuming, as I did assume, and if not, pray correct me, that your system was not a new system, but that you adopt all the different means known to the medical profession generally, and inhalation, but that you insist on inhalation more than others do?—I insisted more than others did on a certain theory of consumption, and on certain principles in the administration of inhalations.

And what new drugs did you propose to use besides those which, as far as you knew, were in use before?—I did not propose to use any particularly new drugs. I proposed to use a new principle—oxygen.

You told me that stramonium was one of those things that you used, and that you considered it new?—It was not new, it was a very old remedy, but it was not used in inhalation.

At the time you published your work you did not believe that stramonium had been used in inhalation?—No; I do not say that. It had been used by smoking, I knew; that it had been used in the patent French cigarettes by asthmatics, I think very likely; but I did not know that, and as I attached great importance to it as a sedative in pulmonary cases, I considered it an improvement in the practice.

Then although you knew that stramonium had been used in cases of asthma in the form of cigarettes?—I did not know that.

Although you believed it possible?—I think it is very possible; and I will go further, and say that I think it very probable.

May I go further, and say that at the time of the publication of your book you did not believe stramonium had been used?—Certainly; it was not in the published *Pharmacopœia* of the Consumption Hospital.

What was the other thing that you believed had not been used?—Chloric acid.

You lay great stress upon that?—Yes, as an oxydizing inhalant.

Let me first call your attention to this. You knew that inhalants had been used in England?—It would hardly be necessary for me to answer that question again, as I have answered it already.

I ask you for a particular reason; yes or no?—Certainly; I knew that they had been used.

You said you yourself went to the Brompton Hospital, did you not?—Yes; when I was here in 1857.

Do you know this (handing a book to the witness)?—It is the *Pharmacopœia* in use at the Brompton Hospital, with those different inhalations prescribed?—No; I do not recollect seeing this work before. I have one which I presume is similar.

I see here opium, iodine, hydrocyanic acid, chloroform, and a number of other things?—Yes, hydrocyanic acid is one of them, and hyocianus is another. The difference is very marked between these and those which I use. If those are the only prescriptions employed at the Brompton Hospital, the whole object is apparently to soothe and allay irritation, with the exception of iodine, which is the only agent used with a view of promoting absorption or removing tuberculous secretion.

I do not disagree with you, but I understood you to say that you use stramonium?—Yes.

You use also conium?—Yes.

And one or two other things you get from the chemist?—Yes; I get what I use from the chemist undoubtedly.

And you use them as inhalants?—Yes.

And you were aware that these different things, or some of them, were used as inhalants at the time you were in this country in the year 1864?—Yes; undoubtedly I knew that.

Now let me call your attention to that which is signed by yourself, Robert Hunter, M.D., at page 36 in the Roman letters, "If anything were necessary to establish the efficacy of the practice of administering medicines in vapour, in all diseases situated in the air tubes, cells, or substance of the lungs, surely enough is furnished in the grateful acknowledgments of those who have tried and proved, in their own persons the curative powers of this treatment. No man of unprejudiced mind can read the particulars of the cases so graphically detailed by Dr. Melville, who, in association with myself, for several years has had ample opportunity of observing the results of this method of practice, or the voluntary testimony of the accompanying letters from patients who have been restored to health, and yet resist the conclusion that pulmonary complaints are curable even after they have reached a confirmed stage, when treated by properly regulated and adapted inhalations of oxygenated and medicated vapours"?—Yes.

Will you point out to me that which shows that there are any particular medicines and oxygenated vapours by which you propose to cure patients?—Well, I will do so. In the first place, I state that there are four classes of medicated vapours; and, in the next place, I state that oxygen is the principle to be employed as an oxygenating agent. That embodies the whole.

I thought you said that as an inhalant stramonium and chloric acid were almost unknown? Do you find those pointed out in your work as some of the inhalants to be used, and the mode in which they are to be used?—No; I merely pointed that out to show that I did not follow the prescriptions of the Brompton Hospital, the effect of which I considered to be purely palliative.

I ask you when you tell people "pulmonary complaints are curable, even after they have reached a confirmed stage, when treated by properly regulated and adapted inhalations of oxygenated and medicated vapours," will you point out anywhere in the work you are giving to the public what oxygenated and medicated vapours you use, and how you apply them?—I did not know that there could be more than one kind of oxygen. I thought that was an element.

What medicated vapours?—But I say that oxygen should be given in such combination with nitrogen or atmospheric air as would meet the circumstances of the case, and as the circumstances of the case could not be known to me, it is scarcely likely that in a book destined

to be read by the public, I should write a prescription that might be adapted to the first stage of the disease, but might be employed in the last.

Do you give any account throughout the work?—It was merely to draw the attention of the medical profession to it. It is not a book written for the instruction of medical men further than to draw their attention to that particular method of treatment and that particular theory. Medical men are at perfect liberty to come to me, and I have extended all the information to such as have that I could possibly give. But this is a popular book.

Am I to take it that you have not mentioned stramonium and chloric acid?—I do not know that I have mentioned even an expectorant by name.

I know you have not, for I have read it very carefully. However, let us go on. "I cannot but regard it as a contribution to medical science of inestimable importance to mankind, and one destined to exert a more beneficial influence on the practice of medicine than any discovery of modern times." Do you adhere to your statement that yours was not a new process, or only in certain instances new, and that you were not representing it as something new?—I do not represent it there as something new. I do not think that any discovery of modern times is half as important as the theory of the carbonaceous character of tubercle, and the oxygen treatment of tubercle. I was not even aware that medical men had to some extent, as I afterwards discovered, dwelt upon that; but their books had evidently fallen still-born from the press, and had not gone through the first edition, and it is not surprising therefore that they had not fallen under my eye. I was not aware that Dr. Birch and many others had written so strongly on the subject of oxygen.

Now go to the next paragraph. "I do not hesitate to say that no such results as those detailed have ever before been effected in these diseases"?—That I conscientiously believe.

"All observation and experience, both in and out of the profession, prove the utter worthlessness of the common routine of fish-oil, sedative cough mixtures, and acid tonics administered through the stomach, on which medical men have hitherto entirely relied." Is it a fact that medical men had entirely relied on those at the time you wrote your letters?—They had entirely relied on those for the cure of the disease. I speak in the general sense—nine hundred and ninety-nine out of a thousand in the profession have, I say, relied upon them.

Do you know that in the hospitals they were actually applying the inhalant in addition to the fish-oil, as you call it?—Not with a view to arrest the disease, but with a view to palliate the different symptoms and the character of these inhalants proves that they had no higher purpose.

You use inhalants for some purpose?—I use them for that purpose. I do not mean to say it is not right and necessary to give soothing and

sedative inhalants; but I say, if you use nothing but them, you do the patient no permanent good, that is, in my judgment, although I may be wrong.

I call your attention to this particular passage, and I ask you whether you meant to say, when you wrote that, that you believed that medical men in England who treated consumption relied entirely upon "fish-oil, sedative cough mixtures, and acid tonics administered through the stomach?"—That is substantially true in every particular at this moment with regard to ninety-nine medical men out of a hundred.

Do you mean that medical men have been in the habit of employing inhalants, and that in the Brompton Hospital in particular they employed them with whatever object they might have?—I can hardly believe that they employed inhalants to any extent, from the character of the prescriptions contained in their *Pharmacopœia*. I find them giving a little conium and creosote, but I call it simply tampering with life. I do not use it as a remedy upon which I rely to cure the disease.

Let us go by steps?—But they have no further steps.

You use remedies which have a soothing effect?—Yes.

Some of your sedatives are tar or conium?—Conium, not tar, unless in bronchitis.

Stramonium?—They do not use it.

Then if you want to destroy the tubercle you must use oxygen?—Yes.

According to your theory you may take sedatives through the stomach?—Yes, to a moderate extent, but it must be to a moderate extent.

These are the ordinary means used by the medical profession to prolong life?—Cod-liver oil is simply nourishment—it is simply animal fat.

Do you think that, in a case of consumption, a thing that ought to be spoken of as utterly worthless?—I do not say that. I say that it is good nourishment, and I say that it may be used for nourishment.

Do you say that it is a very valuable agent, indeed, for the purpose of nourishing a person in consumption?—Yes, the same as beef-tea and beef-steaks, which are also very valuable agents.

Now let me go to the next page. At the bottom you will find this:—"Why, then, in the name of reason, should medical men persist in its employment, or the unfortunate sufferers from lung disease be kept in ignorance of the truth? The issue is one which, to them, involves life itself; and no consideration can justify the failure of the profession to change the practice, or confess their utter inability to combat the disease successfully. In the following pages I have pointed out the method of treatment under which the foregoing cures were effected. The principles upon which it is based are so plain and common-sense, that none can fail to understand them perfectly. It is thus that I would appeal to your judgment. You have the principles of the treatment I would recommend, and you have the testimony of patients who

have been restored to health. I doubt not you will rise from the investigation with a firm faith that, however incurable consumption may have proved under the old system of treatment, it is perfectly amenable to properly regulated inhalations of medicated vapour. Still, you must not imagine that henceforth a new order of things will be immediately established, that the great body of the profession will at once adopt my views and adapt their practice to the principles I have laid down. No amount of reasoning or weight of testimony will lead them to do that. We may convince them of their error, and demonstrate in a thousand ways the superior efficacy of the new system, but we cannot hope to make them publicly confess the fallacy of their practice." That old system is, among other things, the inhalants, the sedatives, at all events?—I do not think that you can say that even that is embraced, inasmuch as the general practitioners of England do not use even those, but in a hospital specially devoted to the treatment of pulmonary diseases, that they should have a few simple forms of inhalation is very natural; but to say that the profession employ them, I do not believe that they do, and I have no reason to suppose that they do.

And that works have not been written pointing out the propriety of using inhalations?—Yes, I think works have been written with that view, but I do not think that those works ever got any circulation or attracted any attention. Certainly they did not attract my attention.

But you are condemning the whole body of the profession, in stating that the present means of endeavouring to cure consumption must necessarily fail?—I think, when you are speaking of the profession as a whole, that when you find nineteen out of twenty, or ninety-nine out of a hundred, or nine hundred and ninety-nine out of a thousand pursue a certain course, and that they really do not know what inhalation is, and do not use it, that then you may speak of the profession as a whole in referring to those examples. I do not say that there are not individual members of the profession who have recently paid a great deal of attention to inhalation, because I know that there are, and I know that the *Lancet* considers that the introduction of a very simple inhaling instrument, which I regard as very inefficient, has formed almost a new era in medicine, and the writer adds that the want of such an instrument is the only reason why the benefit has been denied to patients. If he considers that contriving a simple instrument, which is as simple as a teapot, begins an important era in medicine, I do not think the profession can have had as good an inhaling instrument as that, or have had any inhaling instrument at all, or that they have even practised inhalation at all.

Do you know how vapour was inhaled at the Brompton Hospital?—It was a tin instrument that was used there, if I recollect rightly.

Do you know how it was in 1864?—No, I do not.

I will read you a few words more from this work. I find on page

13, that it says: "Some years ago it became fashionable to treat this affection by applying to the diseased parts a strong solution of nitrate of silver, or lunar caustic, with a probang and sponge. These applications occasion great suffering to the patient, and burn and disorganize the mucous membrane, without accomplishing a tenth part of the good attained by milder means. Where the larynx has become affected, it is both cruel and dangerous to force into the delicate organ of the voice, designed by nature only for the reception of air, strong, irritating fluid caustics. Fully one-half of the consumptive patients whose cases have come under my care were previously treated for diseases of the throat by these caustic applications, and, as they assure me, without any permanent benefit. Indeed, many of them do not hesitate to attribute the disease of their lungs to this practice of burning their throats." Do you mean to say that, as a medical man, you believe those representations?—I believe them conscientiously.

That by using caustic to the throat you produce consumption?—I believe that by the application of caustic to a diseased throat you do establish an inflammation of the mucous membrane, which extends onwards to the lungs, and I believe that all inflammation that arises in the upper part of the mucous membrane may extend gradually down to the lung; and I believe that that is a well-established fact in medicine.

I may take it from you that you desire to have it advertised that if persons treated by the medical profession, when they have a sore throat, are treated with caustic, that it will produce consumption?—I am speaking of a particular form of sore throat—*granular* sore throat.

That that will in all probability bring on disease of the lungs?—It does continually do so. Simple catarrh of the nostrils does, which is much more simple.

I see, again, that in page 21, you refer to the poor, unoffending stomach being treated for disease of the lungs. You say: "There is no use in torturing the poor, unoffending stomach for an inflamed condition of the vocal chords of the larynx."—Yes.

It is all to be done with a particular instrument that you use?—The whole profession may use it if they like; it is a boon open to them; there is no restraint upon it, and, as I have reason to believe, they have recently used many thousands.

Let me call your attention to this, which is instructing the medical profession: "The patient must use the inhaling instrument morning and evening, charged with such sedative, alterative, and astringent medicines as may be indicated by the stage of the disease, and fumigate the larynx every night before going to bed with warm sedative vapours, or he will go on from bad to worse, until cure is impossible. There are few cases so bad that these means will not restore to health, and gradually bring back the tone of the voice. The only cases in which they fail are such as are already complicated by extensive tuber-

cular depositions in the lungs. Cure, then, depends on the possibility of restoring the lungs. When I come to speak of the varieties of consumption, I shall have occasion again to refer to this latter form of laryngitis."—Yes.

Then the only system is to take the instrument you use, and some sedative or astringent medicated vapour according to the character of the disease?—I do not say that they shall take the instrument that I use, but that the principle of treatment shall be local treatment applied to the larynx itself: that is, the inflammation is local and confined to that particular part, and that the application must be in the form of vapour, and that it must be applied by means of inhalation, and that the inhalant must be sedative to allay irritation, and astringent to arrest secretion. It is a very important matter to arrest secretion.

What are you to use?—Astringents and alteratives. I leave the medical men to use their own judgment as to the particular form of sedative, or the particular form of alterative they may make use of.

What is your new system. Is it merely to inhale the remedy instead of taking it through the stomach?—I do not think taking it through the stomach.

I ask you what is your new system. Is it merely to inhale, instead of taking it through the stomach?—I think that every medical man who writes upon the disease—

The Lord Chief Justice. Cannot you say "yes" or "no" to that question? We shall never get to the end of the cross-examination if you make a speech in answer to every question.

Mr. Karlake. It is a very simple question. What is your new system? It is merely to inhale instead of taking through the stomach the remedies to be employed?—Undoubtedly, that is what I mean.

Then I want to know whether you point out anywhere what is to be the sedative or the astringent to be used?—No, undoubtedly; it is not usual to do so.

The Lord Chief Justice. You recommend topical instead of constitutional treatment?—In that particular case I would not say that inhalations are a topical treatment, because they are topical and constitutional as well.

The Lord Chief Justice. Since the oxygen that you use goes into the system?—Yes; the sedatives also go into the current of circulation, and they are absorbed, as Professor Carpenter says, with surprising rapidity.

Mr. Coleridge. Here he is dealing with chronic laryngitis.

Mr. Karlake. I see you give a good many pages to that.

The Lord Chief Justice. In order that the remedy should soon form a vapour, it must be warm?—It must be warm. Certain substances may be inhaled as cold vapour, but the majority must be inhaled from hot water of a high temperature.

The Lord Chief Justice. That is the difficulty; is it not?—No; I do not think it is an objection.

But by the use of warm remedies you relax the part generally ; do you not?—My lord, I think all authorities upon pulmonary disease agree that the vapour of warm water is most soothing and salutary.

The Lord Chief Justice. But you were speaking of the larynx and those organs higher up.—The same principle would hold good ; the vocal chords being lined by a mucous membrane, the soothing effect of warm vapour is very beneficial. I can only say with reference to laryngitis, probably my language has been a little too strong ; but if so, it arose from the fact that laryngitis has increased, at least within the last twenty-seven years, about 1500 per cent.—that is, the fatality of it.

Mr. Karlake. Now let us take some symptoms. Page 61 treats upon galloping consumption. “There are few symptoms by which this disease can be distinguished from a low form of inflammation of the lungs. Indeed its symptoms are so unlike those of the common form of consumption that its nature is generally denied until after death, when, upon examination, the whole internal surface of the air-tubes, the air-cells, and even the substance of the lungs, are found thickly studded with tubercles. This disease is most common in young persons between the ages of fifteen and twenty-five, of full habit, clear complexion, and ruddy cheeks, though it sometimes occurs much later in life. There is another form of consumption, also rapid in its progress, which occurs in young persons of delicate constitution, who have habitually cold hands and feet. These have commonly an inherited predisposition. Their air-circulation is feeble, and they become weak, fatigued, and out of breath on every slight exertion. In such cases the destroyer steals silently upon his victim—the patient scarcely feels ill—there is but little cough, no pain, no spitting of blood, often no expectoration. Having always been short-breathed, and liable to colds, when cough does occur it is attributed to a little fresh cold. Gradually the countenance becomes pale, the lips lose their colour, and the eyes are sunken, with a dark or bluish line beneath them. Under these circumstances, without any increase of cough or expectoration, diarrhœa occurs, and the patient dies without a struggle, perhaps sitting up in a chair, or in a fainting fit, from which friends strive in vain to revive him.” Are the symptoms which you describe there symptoms of many other things besides galloping consumption?—Yes; cold hands and feet I look upon as a very significant indication.

Is that or not a description of one hundred other things besides consumption, or an approach to it?—It is a description very similar to that given by Sir James Clarke, a very great authority on the subject.

Is it or not a description of a hundred other things besides consumption? Say yes or no.—Every symptom which occurs in disease of the lung may occur in other diseases.

I am much obliged to you for granting me that. Then the person may have cold hands and feet and afterwards faint in a chair, and there

may be nothing more than cold hands and feet, and it may all be the result of a cold?—I do not know that.

Perhaps you will tell me what it is?—It is evidence of defective circulation, of imperfect vitalization of the blood, and of imperfect circulation of the blood, and most likely results from a peculiar condition of the lung; and every authority, the very highest that has ever written on the subject, gives that very description which you have just read of galloping consumption.

That is the answer you give?—Yes.

Is the inference that I am to draw from that this—that a young person having cold hands and feet should come to Dr. Hunter?—No; they should have the chest examined, if it is *habitual* cold hands and feet.

Now with regard to latent consumption. “The books describe these cases under the name ‘Latent Consumption,’ because the tubercles are in a latent or quiescent state, producing neither cough, expectoration, nor spitting of blood. There are in this form of consumption no symptoms which directly point to the lungs. The practised eye of a physician of experience——” As I understand you from previous passages there are none?—I do not know that. Very good diagnosticians may go wrong at times.

“A physician of experience in the treatment of pulmonary diseases would probably detect in the countenance what would lead him to carefully examine the chest; but the general practitioner having only a limited experience in the physiognomy of consumptive cases, would see nothing beyond the fact that the patient was slowly declining in energy and bodily vigour. As there is no cough.” Is this an adaptation from Sir James Clark, do you recollect? Just look at page 64.—I could not tell you that. Descriptions of diseases are very liable to be similar. One description is very much like another; the language is a little different, but we are obliged to describe in the same way. But I will stake my reputation on the fact, that Sir James Clark gives a very similar description.

Do you happen to recollect that in Sir James Clark’s book the words of this passage are—“In these cases there is sometimes no cough,” and not “as there is no cough?”—I do not know.

“As there is no cough, no pain in the chest, no expectoration, the lungs would probably be overlooked altogether, until some one of these symptoms arose; and that, in latent consumption, means not until the last stage sets in, and the end is near at hand. A gentleman came to consult me the other day whose disease was of this character, and yet it had never been suspected by either his physicians or friends. Had he not accidentally taken cold and suffered from a slight pain in his side, he would not have even thought it possible his lungs could be diseased. On examining his chest, I found the superior lobe of the left lung filled with tubercles;” and so forth.

Mr. Coleridge. He says, “in a latent state.”

Mr. Karstake. "On examining his chest, I found the superior lobe of the left lung filled with tubercles in a latent state. This gentleman had been in poor health several months, but nobody could tell him why he did not get well. He had consulted half a score of doctors, and got almost as many different opinions. Some treated his case very lightly, and promised a speedy cure by a course of tonics; others advised change of air and patience. One told him that it was all stomach; while another assured him that his liver was out of order. He tried one after another, but obtained no benefit from any. Those who knew the least about his disease promised the most, but accomplished nothing; while if any suspected the lungs as the cause of his bad health, they did not enlighten him. I would not from this case argue that the majority of physicians are ignorant of the means of diagnosis, but that their experience is limited, their time so occupied, and their minds so distracted by attendance on acute diseases, that they are extremely apt to overlook and maltreat chronic affections of this nature;" so that you must go to some one who makes it his speciality?—I believe that is the case.

All these doctors made a mistake. One of them said that it was his liver, and some that it was his stomach; but Dr. Hunter gave the correct opinion?—Undoubtedly; I have done so many times.

This is another symptom, with variations, at page 66: "And Sir James Clark, in speaking of the liability to mistakes of this character says: 'I have known more than one example of extensive tubercular disease of the lungs discovered by a *post-mortem* examination where, during life, the disease was looked for in the stomach, liver, or bowels.'" Is that a quotation which you made from Sir James Clark?—Yes.

We will have Sir James Clark, and see if we can find that in his work. 'From these facts you will understand what a mistake those persons make who flatter themselves that they are in no danger of consumption, because they have never had cough, pain in the chest, or expectoration. Thousands who have never had these symptoms are to-day carrying about in the lungs the seeds of their own destruction. Whoever finds himself less vigorous and capable of exertion than formerly—and, indeed, all persons who are in bad health, without knowing why—should suspect the lungs as the cause, and never rest satisfied until they have submitted them to careful examination. I know of nothing which should more strongly excite their fears than being told their liver is diseased. Experience has proved that the great majority of such cases turn out in the end to be mere cases of masked, or latent consumption." So that if you feel a little out of sorts, and your doctor attributes it to the liver, you must attribute it to the lungs?—I think they should be examined.

Dr. Hunter is to make the examination?—No; there are plenty of my medical brethren standing round here, who are quite as competent to make a physical examination of the chest as I am.

Excuse me, but you say at page 65 that their minds are so distracted that they cannot attend to that?—They are quite as capable as I am to do that.

Mr. Coleridge. You will find, if you read on, “Had the stethoscope been applied to the chest”——

Mr. Karlake. “Had the stethoscope been applied to the chest, it is probable that some of them might have detected the presence of tubercles; this was not done, and but for the accident of taking cold, it is almost certain the disease would have gone on to disorganization of the lungs and the sacrifice of the patient’s life.”—I may suggest that Dr. Hughes Bennett, of Edinburgh, gives a great number of cases of that kind as illustrations in his work, and it will be interesting probably to read them in connexion with this.

Mr. Karlake. To whom? To the persons who read the advertisements, because you do not tell them anything about that work?—If I say what Professor Bennett says on that subject—I consider if he is a respectable, honourable, and conscientious man, that he is only so on precisely the same ground.

Did he advertise to the general public?—I do not know that the facts are altered by being generally known. I believe that is a merit rather than otherwise.

You think it rather a merit than otherwise?—Yes.

Now at page 69 we find, with regard to consumption in children—“When I say, therefore, that comparatively few physicians are able to pronounce a safe opinion on the health of the lungs. I mean simply that medical men engaged in general practice cannot acquire the great experience which is necessary to enable them to detect disease in obscure cases and in its early stages. Any tyro in medicine can detect consumption in the last stage; but a medical examination, to be of any value to the patient, must discover the presence of tubercles, if they exist, before the lungs are seriously injured, and while cure is yet possible.” That applies to children particularly?—No, not necessarily to children. It shows the importance of examining the chest.

Let us take another, from page 71: “In children this disease does not manifest itself by active symptoms. There is no spitting of blood, or, if so, very rarely. Where cough occurs, it is in paroxysms. There is no expectoration, for all the matter forced up from the lungs is swallowed. There is, commonly, moisture on the forehead during the night, but seldom a distinct night sweat; some fever during the day, but it is generally ascribed to worms. Diarrhœa is a very common symptom, but it is almost sure to be attributed to indigestion, or thought to be a mere summer complaint. There is one symptom which is always present—viz., gradual wasting away; but it is impossible to detect consumption in children by the general symptoms. The stethoscope is the only safe reliance. I always suspect the health of the lungs in children when I observe them subject to any of the following symptoms—Eruptions about the face, puffy and chapped lips,

pustules on the edges of the eyelids, eruptions behind the ears, soreness and running from the nose, swelling of the glands of the neck, and gradual wasting of flesh and strength." Are not these the symptoms much more generally the symptoms shown in cases of scrofula?—They are considered by most authors to be identical.

Do you mean many people are not subject to scrofula who are not subject to consumption at all?—Undoubtedly; but they are very liable to consumption.

The Lord Chief Justice. And *vice versâ*?—Not *vice versâ*.

Do you mean that no persons have consumption, and die of it, without showing any evidence of scrofula?—Undoubtedly; but I do not believe that consumption leads to scrofula, but the reverse.

Mr. Karlake. Do you mean to say that these are not also symptoms of scrofula?—Undoubtedly.

"None of these are necessarily signs of consumption; but they show a bad state of the general health, which in a great many cases will be found to arise from the presence of tubercles in the lungs. An examination will always put the matter beyond doubt, and, consequently, that parent who neglects to have it made is guilty of great cruelty and neglect." In all these cases they are to go to the doctor immediately and have their chest examined?—It is a mere argument in favour of the examination of and attention to the children. If you knew the prevalence and fatality of infantile consumption, you would realize that, probably.

I have read the statement as I find it there.—As to the prevalence and fatality of the disease?

Yes; and I read the next, where you say it is not constitutional, and it cannot be constitutional.—No.

Do you not talk of there being a predisposition, but it cannot be constitutional and be inherited?—I do not say that it is not possible for consumption to be constitutional and inherited. I say that it is often inherited.

Is that not a statement of yours?—I say that a child born of healthy parents, whose mother has no evidence of the existence of tubercular matter in her system, does not necessarily inherit the disease, but he may inherit the predisposition to it; and that every moment he draws his sustenance from the mother, if she have tubercular disease, he may be drawing it from her. Ail he can have is the organism at the time of birth. That organism may not be diseased, but the sustenance that he draws may predispose it very strongly to the disease.

May I take it that, according to your judgment as given in the book, it is not constitutional?—If the parents are perfectly healthy, the child cannot inherit consumption.

Although the parent after it is born dies of consumption?—I am assuming that she had not actually tuberculous blood.

Assume it.—We cannot assume it. It is probable, if the child

manifests disease shortly after birth, or admitting that the mother has the disease, that the child inherits it.

Mr. Coleridge. Some females are more liable to consumption than others.

Mr. Karslake. You set up the predispositional theory, but deny the constitutional theory.—No. I say that the constitutional theory accounts for a certain limited number of cases, and that the predispositional theory accounts for other cases, and that the acquired cases are much more numerous than both the other cases put together.

You say that you do consider that it is constitutional; may I take that to be your answer?—That it *may* be constitutional?

That it may be constitutional, but that it is ordinarily *local*.—That is a term that I do not use at all.

What do you use?—I say that consumption arises. The term consumption is a term applied to tubercular disease of the lungs by a sort of general consent. At the same time, any tubercular disposition is really a consequence of a certain impure condition of the blood, and that that is incident to the deposit of tubercle; but *pulmonary* consumption may be a local disease originally, but as consumption depends on a certain condition of the blood, and as that blood circulates through every organ of the body *tubercle* is liable to be deposited everywhere, and all cases are constitutional after they are once established.

At page 90 you say that “tubercles are the consequence of imperfect oxygenation of the blood; their base is carbon. They may be inherited, but are generally acquired. They are produced equally in mankind and in brutes by whatever diminishes the purity of the air or the freedom of admission into the lungs. The body wastes in this disease, because the quantity of chyle assimilated is regulated, to the weight of a grain, by the quantity of oxygen supplied to the lungs”?—Certainly.

Now, I think that we find at the bottom of that page a statement upon which I want to ask you a question, “I have said that the blood is always impure in consumption. What is the nature of the impurity? Chemistry demonstrates that the oxygen is always diminished and carbon always in excess. Carbon being a poison most inimical to life, I regard it as the true cause of tubercle.” Is carbon most inimical to life?—I think so.

You really mean to say that?—I mean to say that free carbon in the blood or an excess of carbon in the system is believed to be the principal source of all *infectious* diseases—all *epidemic* maladies.

Is that the view you take, that free carbon is the cause of disease, and that in the case of tubercle, in point of fact, tubercle is carbon?—I mean to say precisely this, that the lung being incapable of eliminating the carbon from the blood, the carbon remains in the system, and forms the *nucleus* of the tubercular matter.

Well, we will go on. “The analysis of Scherer proves that 54 per

cent. of it is pure carbon, and the remaining 46 per cent. made up of the elements of disintegrated tissues of the lungs in which it is deposited." Did you consult Scherer before you made that statement?—Probably not Scherer's work, for I think it is published in German, but it was an extract.

Is the object which you desired to attain by the inhalation of oxygen the destruction of that 54 per cent. of pure carbon?—It is to produce a vital condition of the blood.

That is going away from what I have asked you. Is the object of your process of inhalation of oxygen the destruction of that 54 per cent. of pure carbon which you assume to exist in the blood?—It is to promote its elimination.

To get rid of it?—Yes.

Is that what you propose to do by inhaling oxygen, which is to impinge upon the lung, and set free the carbon?—I propose to do more. I propose to purify the circulating blood, and to give tone and vitality to the liver.

No, no, but I ask you when you make that statement at page 90, "that the blood is always impure in consumption. What is the nature of the impurity? Chemistry demonstrates that the oxygen is always diminished and carbon always in excess. Carbon being a poison most inimical to life, I regard it as the true cause of tubercle. What does chemistry tell us of the essential nature of tubercle? The analysis of Sherer proves that 54 per cent. of it is pure carbon, and the remaining 46 made up of the elements of disintegrated tissues of the lungs in which it is deposited" whether, when you prescribe, as you say you do, the use of oxygenated vapour, which is to be taken in such a form as shall communicate directly with the lung, is it for the purpose of destroying or oxidizing, or getting rid of the 54 per cent. of carbon which you say exists in tubercle?—Or causes its absorption.

The Lord Chief Justice. If I understand your theory correctly, it comes to this, that as soon as the lung appears to be in a state of disease, that it cannot inhale by natural respiration the quantity of oxygen necessary to get rid of the worn-out tissue, and that consequently, instead of the carbon being got rid of by the oxygen, which in a healthy subject would be inspired in a natural way, you want to supply artificial means, which cannot be got into the lung naturally?—That is part of my theory, my Lord.

What is the rest of it?—That is not the whole object, because there is a vital tone in the system upon which the health would depend, and which must be restored.

Is not that by the introduction of the necessary volume of oxygen?—Yes.

You say, when the lung is in a state of disease, it cannot take in that quantity of oxygen by respiration?—Without local disease the lungs may be in such a condition as to be unable to take in a proper quantity of oxygen, and yet tubercular disease of the lung not be established. The

tubercular disease may be a consequence of that. The one supposition would require that the disease should exist before the impurity of the blood, which would be reversing the thing.

You say that the tubercular deposit is the result of previously established disease?—It is the result of a previous carbonaceous condition of the blood.

Which you say is brought about by the imperfect condition of the lung?—Now necessarily; confinement in an impure atmosphere, and cramped position of the body. A local irritation, by diminishing the calibre of the tubes will produce the same condition.

Do you say that tubercular disease does not develop itself spontaneously, that it is not the same thing that establishes itself there, and a cause instead of being the effect of the disease?—I think it is possible in some instances that it is so; but I believe as a rule that it is acquired, and that impure air and cramped position of the body, or the existing predisposing causes, lead to a certain condition of the blood, and that tubercles are the consequence of that condition.

Then you must restore the purity of the blood by artificial means?—Yes.

Mr. Karlake. When people have asthma, and many diseases of a painful character, do you not find many of them die without the slightest tubercle in the lung?—Yes, undoubtedly.

But although the air passages have been obstructed to a considerable extent, the lungs are unaffected?—Undoubtedly; but they are generally people with very large lungs.

Do you find, particularly after death, some people have had tubercle in the brain as well as on the lung?—In every organ of the body.

Then I put the question to you that I put before, and I am very anxious to have your answer, whether you do not base your theory upon there being 54 per cent. of pure carbon in the tubercle, which is to be destroyed by the excess of oxygen that you are able to introduce by inhalation?—I base it upon that and other facts.

I see you say immediately afterwards—"If the reader has attentively followed me through the causes of consumption, he will now understand that the first step towards its cure must be an effort to restore the obstructed function of the lungs. The blood cannot be restored to its purity until this is done." Is the blood restored to its purity by medicine taken through the stomach?—I do not know any medicine that has that power.

Will not anything do it?—To a moderate extent.

Poisons get into the system through the blood?—Yes.

Is not the blood purified by these medicines that are administered?—It may be purified by stimulating the functions.

The Lord Chief Justice. Is not acid given with that view?—Acid is given chiefly for its action upon the stomach, upon the presumption that the stomach requires the tonic powers of acid to stimulate it.

The Lord Chief Justice. Is not that because the oxygen which the

acid contains being set free, passes itself into the blood?—To a very limited extent.

But that is the principle upon which acids are given?—To some extent. Their immediate effect is, of course, tonic. All acids being astringents, give tone to the membrane, and cause a more healthy secretion of the gastric juice, and so improve the appetite.

Mr. Karlake. Does it not actually improve the blood?—I do not know that oxygen is absorbed in a state of gas in the stomach.

I ask you whether the blood is not affected by the medicines that pass through the stomach?—It is affected to a certain extent.

Is it rendered more healthy?—That depends on what way you put the question.

The Lord Chief Justice. If you improve the functions of digestion, as he suggests, and the power of assimilation, of course you improve the condition of the blood.

Mr. Karlake. I want to know whether from using those medicines in the stomach, you do not improve the blood?—You increase the pabulum out of which the blood is made; but the transmutation of the chyle into blood depends upon the volume of oxygen in the lungs. The last act of assimilation is the oxygenation of the chyle and lymph in the capillaries of the lungs, and therefore you may give too much nourishment, and it may be eliminated by other organs. You may give particular kinds of nourishments, and they will increase the fat of the body; but the condition of the blood depends still on the amount of chyle and lymph that can be *vitalized* or assimilated by the respiratory function.

Is it the blood itself that must be cured of this excess of carbon?—I think so decidedly.

Do you mean to say that no good is obtained from fatty food?—No good—I do not use that term. I think that fat, for example, especially animal oil, is readily absorbed; and I think it is an exception. It does not, like the albuminous elements of food, undergo oxydation in the lungs, but floats as free uncombined oil, and is deposited as adipose tissue; it therefore accounts for a person gaining in weight while the disease goes on—he may be making fat all the time, but it does not sustain his body. It does not arrest the disease.

Does it prolong life?—Sometimes it does, and it is, I think, good nourishment. It is very difficult to say how far it prolongs life.

Then, it being very difficult to say, I find you say in page 95—“I have designedly taken the above facts from the highest and most trustworthy sources known to the profession, because they point out something more than the mere average duration of consumption. They show us the almost certain result which follows the wretched mockery of pretending to treat this disease through the stomach.” Then there is Baron Louis, who gave his experience, which you cite there?—Yes, and Bayle, and Dr. Swett, as to the mortality and to the progress and

duration of the disease. I have selected these facts from those distinguished authorities.

And it is the miserable system of treating it through the stomach which leads to those results?

Mr. Coleridge. He does not say that.

Mr. Karlake. "They show us the almost certain result which follows the wretched mockery of pretending to treat this disease through the stomach"?—Yes.

I take the words which are used by yourself. Then your theory is still more strongly put in Letter 21 at page 112 :—"From these facts the reader can understand that one of the great objects of treatment is to subdue this catarrhal condition of the mucous membrane. How can this be accomplished? It is no treatment for a local disease in the lungs to pour cod-liver oil and tonics into the stomach, for they never reach the part affected." I suppose we may assume that they do not get into the lung bodily—"and besides such medicines possess no properties capable of effecting cure if they did. No physician will pretend that cod-liver oil, or any cough mixture or tonic ever compounded, has power to remove tubercles, purify the blood, or heal the mucous membrane, even if directly applied. How, then, in the name of reason, can they accomplish these objects when they are applied to a distant and a healthy part? They are merely palliatives—nothing more—and nobody, unless it be the patient, expects them to heal the lungs." So that they must be treated locally according to your view?—I do not say that. I have yet to know that the inhalation of oxygen is a local remedy, or that the inhalation of anything that is taken up and carried into the circulation is a local remedy. It is no more local when taken by the lungs than by the stomach. It is a different channel only.

The Lord Chief Justice. In relation to sedatives it would be local?—To a certain extent; but even they are partially taken up.

Mr. Karlake. Do you agree that oxygen actually communicates with carbon in the lung?—It communicates with the blood. I think it is possible to produce the eliminating effect upon the mass itself; but that is a matter that we have no earthly means of demonstrating. We are satisfied with results—the practical results—which are evinced in the absorption of tuberculous matter, and by the removal of tuberculous matter followed by cicatrization of the lung.

From the use of oxygen?—And other inhalations.

Then we have the fact admitted at page 114 :—"But, it is frequently asked, do you give no medicines by the stomach? Certainly I do—when they are required. If the patient is feeble and losing flesh, I put him upon strong, nourishing diet, and give tonics to aid his digestion. There is no objection to the use of cod-liver oil as nourishment under such circumstances, although I find rich soups, made from fat meats, quite as beneficial and a great deal more palatable. If the bowels are confined, the liver torpid, or the

function of any other organ impeded or suppressed, medicines must be given to correct them?—Undoubtedly.

Then I assume that you do use all those commonly in use throughout the profession generally?—Yes, through the stomach because they are intended to act through those parts.

Now I am not sure whether I did not pass over the symptoms of consumption. There was a passage which I intended to read about the symptoms of women. You will find it at page 46. “The losing a little flesh is often an early symptom of consumption. It takes place in many cases even though the appetite remains good and the food is abundant and nutritious. When a patient grows thinner without any appreciable reason the lungs should always be suspected as the cause. The same is true of those who rapidly gain flesh and then as suddenly lose it.” So that in either view it is the same. “When a patient grows thinner without any appreciable reason the lungs should always be suspected as the cause. The same is true of those who rapidly gain flesh and then as suddenly lose it. These changes merely indicate the changes taking place in the lungs. Still we do not always find apparent loss of flesh in the first stage of tubercle. In young women particularly I have very often found the lungs seriously affected while they still retained their colour and plumpness.” That is the young women, I suppose. “But, as a rule, if we reduce the matter to a certainty by weighing, we shall find a few pounds of difference between their present and former weight. If with the loss of weight, there is a disposition to sigh, a dark discoloration below the eyes, and a quickened pulse, with some heat in the hands, set it down as almost certain that the lungs are affected.” Are not those symptoms, symptoms which are constantly exhibited in young women without their having anything like lung disease?—Constantly.

Frequently?—If there is a constant disposition to sigh, it is an indication of want of breath. We are speaking now, you must recollect, of habitual indisposition which does not arise from normal causes. Discoloration below the eye is an evidence of bad circulation.

And it is an evidence of many other things?—It might arise from other things.

That is all I say. “Still I have, in a few instances, known persons to gain flesh during the whole of the first and second stages of consumptive disease, and that even where the tubercles were rapidly undergoing changes which, if not arrested, must soon have ended fatally. Strong nourishment will sometimes improve the weight of persons who are hopelessly affected, though the increase in weight exerts no beneficial influence on the disease itself. The irritation produced by tubercles, even in the first stage, will sometimes give rise to a sense of chilliness followed by slight fever,” and so on?—Yes, hectic fever.

Then I see in the next page, 48:—“In young females irregularities almost always occur sooner or later, and often this is the first and only evidence they have of disease?”—That is sooner or later in this

particular disease that irregularities are liable to cause this particular disease of which we have been speaking.

Yes. "We examine the lungs and are surprised to find them the seat of miliary tubercles. Too frequently in such cases strong medicines are given to restore the suspended function, and the careless physician only discovers his cruel mistake when his administrations have aggravated the pulmonary disease. The tubercles are then attributed to the suppression, when in reality they are the cause which produced it. When any doubt exists, the proper course for a patient is to submit the lungs to careful examination by a physician specially skilled in pulmonary cases. It would be too much to expect the medical attendant to confess an error of diagnosis, and yet few are so learned and experienced that they do not make many mistakes in the early stages of obscure cases, and in distinguishing between purulent bronchitis and in tuberculous ulceration of the lungs. Much of the mortality caused by consumption results from the hesitation and delay in adopting a proper treatment in the early stage. And this evil will continue so long as medical men resist the division of medical practice into specialities. It is only by devoting exclusive attention to a class of diseases so intricate and important as those of the lungs, that the physician can acquire the experience essential to correct diagnosis and successful treatment. I mean no disparagement of my professional brethren when I say that the general practitioner, however skilful, has neither time nor sufficient opportunities to become a good stethoscopist, and yet while he continues to treat consumption the lives of his patients hang upon his skill in this respect. The remedies employed depend on the stage and form of the disease, and no physician can successfully treat a patient while he is in doubt and uncertainty as to the nature and extent of the pulmonary affection?"—Undoubtedly.

"In all cases where there are any of these symptoms, suspect pulmonary disease; do not trust to the general medical practitioner, but go to some person who makes it his special object?"—I think so.

And, of course, there is no difficulty in going to 14, Upper Seymour-street?—No difficulty.

Now, when did you commence administering oxygen to patients?—In 1851 or 1852.

In what way?—A variety of ways.

Give me one.—By chlorate of potassa, and peroxide of manganese, I produced pure oxygen gas.

How was that?—It was collected in a gasometer suspended by weights.

That was in what year?—1851 or 1852.

That was the result of your experiment?—That was one of the means I employed in the treatment of cases, after I decided upon devoting myself to the treatment of pulmonary diseases. It was not the

means I employed in the treatment of my own case, for it was impossible for me to avail myself of pure oxygen gas at that time.

What were the means you used, then?—I converted the gas in small quantities, and collected it in an India-rubber bag, and breathed that.

How?—In a small bag. Protoxide of nitrogen at that time was more convenient.

More recently, what has been the mode by which you have produced oxygen?—I experienced the fact that oxygen does not produce the same decided effect when given in its pure form as it does when given in combination with nitrogen; and the use of acids—strong acids, acids which contain a very large volume of oxygen, such as chloric acid and nitric acid.

When did you discover this?—Many years ago; it might have been in 1854, or about that time. I began to use acids in the inhalants within a year or so from that time, and it was the result of a series of investigations and experiments.

What did the result end in? What was the gas that you found most beneficial to the patients?—what mixture?—Chloric acid, a drachm to the dose is what I relied on principally of late years, because I found that chloric acid liberates chlorine as well as oxygen, and is probably more efficacious in the majority of cases; but dilute nitric acid liberates nitric acid vapour, which is acceptable to the lungs. The oxygen is taken by the blood in the same manner as the fumes of acid brought in contact with any metal would give up its oxygen. The affinity of the blood for oxygen separates it, and good results follow.

That you discovered when?—The last ten years.

What is the practice of the last ten years, then?—I use oxygen in a gaseous form; then I liberate the oxygen by the burning of a pastille in a little metal cone filled with nitrate of potassa, stramonium, and other ingredients.

Is that your invention?—Yes; I am not aware that it could be got in London to-day unless you came to me for it. I never heard it recommended as a remedy before.

By what means is it that you enable the patient to inhale these gases?—It is diffused in the chamber in which the patient sleeps.

What are the modes in which you have that done?—By the decomposition of the nitrate and the combination of the stramonium, you liberate the oxygen with the stramonium.

That does not require an inhaling instrument?—No.

What inhaling instrument do you use?—A glass inhaling instrument.

Of an ordinary character?—It is a little different from all other inhaling instruments.

The difference is what I want to know?—The same principle appertains to all. It is merely the manner of inhaling.

I think I find in the *Lancet* a sort of triumph displayed at the discovery of an inhaling instrument?—I think that that inhaling instru-

ment is very much on the same principle, although the air is drawn over instead of through the fluid, and then the medicines are placed in a sponge, instead of in the fluid itself.

Were you the inventor of the instrument that you use?—Yes.

Those are the instruments sold to patients?—Not sold, but furnished.

Do you find that one advantage of your mode is, that people need not come to you that can describe their phenomena at a distance?—That appertains to all medical men.

Is it peculiar in your practice?—No; they can consult by letter, giving me the diagnosis that has been formed by their medical man, and their symptoms. They can undoubtedly consult me by letter.

I thought you said that their medical man, not having special knowledge, would be almost useless?—I form my own opinion from the symptoms of the patient, and the symptoms described, whether I am to attach importance to it or not. The instrument was discovered ten years ago.

But you say here:—"Some seem to suppose, that as practised by us, it is a mere adaptation of Ramadge's inhaling tube for the exercise and expansion of the chest and lungs. With this instrument atmospheric air alone is employed, and considerable exertion is required to use it properly. Whereas we employ a variety of apparatus, adapted to the requirements of the several cases under treatment, requiring little or no effort beyond the ordinary act of breathing, and we mingle with the air medicinal substances, rendered respirable by pharmaceutic process, and presented in the gaseous form, as dry or moist vapours, and as spray. Thus exhibited, medicines are deprived of many of the disagreeable concomitants which attend their administration by the stomach. Not a few think that in order to avail themselves of this mode of treatment, they must do so at the sacrifice of an absence from home and its comforts, and a consequent increased pecuniary outlay. This is altogether unnecessary. The treatment of chronic disease of the pulmonary organs by this method, can be directed and carried on quite satisfactorily without the constant supervision of the physician. Some of the most gratifying cases as to result have been thus treated by us entirely by correspondence, on statements of the history and condition of the patient given by themselves or their ordinary medical attendant." It is not necessary that they should consult him for the purpose of using this?—Not in chronic diseases—it is not necessary to have a regular attendant. It is necessary to have that advice to report their progress and symptoms, but they do not undergo change in a few days, as acute diseases are likely to do.

Do you mean to say this inhalation can go on without the superintendence of yourself or one of those gentlemen who have studied the system?—Yes, without the professional superintendence, because they state by letter what they would state if they came personally.

Then they are to use what new gas you tell them?—They are to increase or diminish the quantity of the inhalant, and if there is any-

thing necessary to render it more effective, a modification of the inhalant is furnished to them.

Is there any other medicines besides those you have mentioned by which you generate oxygen?—By the decomposition of the nitrate of potassa, and the chlorate of potassa, and the inhalation of acids. Sometimes we use oxygen in combination with atmospheric air, and we have used pure oxygen.

And how is that applied?—As I stated, it is applied in some instances by generating and collecting the oxygen in a gasometer. We use here a small gasometer as being more convenient, containing compressed oxygen.

May I take it that there is not in any one of these letters a description of that mode by which you propose to use the oxygen?—Undoubtedly. I did not think in a popular work of this description that it was necessary.

I ask you whether you mean to say that there is nothing to indicate the mode by which you propose to cure, or to remedy to a very considerable extent, that pulmonary disease?—There is quite enough to enable a medical man to understand perfectly the principle.

I am not talking of the principle, but the practice.—But I find no more in the works of other men.

Is that an answer to my question?—Undoubtedly; there is quite as much as is given by medical men in writing their prescriptions, when writing for themselves.

Is there nothing that points out the mode in which you apply these remedies?—The particular description that I employ, do you mean?

Yes.—No; I do not know that medical men write prescriptions in medical works, as a rule, even when the works are prepared for the profession.

The Lord Chief Justice. Should not the inhaler used be explained?—An inhaler is just as well known to other medical men as to me. Every medical man understands them perfectly.

Every medical man knows what they are if they are to be applied in the usual form, but if they are to be administered in a form hitherto unknown, it might not so readily occur to them.

Mr. Karlake. That will be a question.

The Lord Chief Justice. It might be more difficult for medical men to know how sedatives or alteratives are alluded to.

The Witness. Medical men, my lord, have called upon me almost continually, and received such information as they wanted.

The Lord Chief Justice. That is the best answer, then, that the book itself does not communicate that information.—It is unusual for medical men to communicate particular knowledge of that kind.

Mr. Karlake. However, such is the fact, that you do not describe it. I only ask you the question. May I take it generally that your views on the subject have undergone no modification since the time of that book being published.—I do not think they have.

The Lord Chief Justice. Are you in practice now?—I am.

Mr. Karlake. You adhere to the views there expressed.—Yes; I adhere to a correct and proper interpretation of them, not exactly to the interpretation you put upon them.

My friend asked you were you editor of a newspaper?—I was.

Were you also proprietor of it?—Yes.

It was called the *Medical Specialist*?—Yes.

For how long was it published?—For little more than two years.

Was it mainly devoted to the cases you yourself treated?—No; I had a staff of contributors in different branches of medicine.

Was it mainly devoted to the inhaling process which you yourself set up?—No; it was mainly devoted to the elucidation of pulmonary disease, and the treatment of various forms of pulmonary disease; but I had also contributions on other forms of disease from medical men.

It was principally devoted to pulmonary disease?—Yes, principally.

When was that given up?—It was given up shortly before the time I returned to Canada; about the time I came over to England and visited the Brompton Hospital.

Who are the gentlemen associated with you at the present time? It took four, I see, in 1864.—Dr. Melville is the only medical man at present associated with me.

Is it a regular partnership between you?—No, not in every sense a partnership.

Does he receive a salary from you, or fees?—He receives in lieu of a partnership, or in lieu of an interest in the practice, a given remuneration.

That is the association that is here referred to with Dr. Melville, Dr. Wills, and Dr. Macgregor. Did they receive the same?—Dr. Wills was never associated with me. Dr. Macgregor did while he was with me in London, but after leaving London he had the practice to himself.

Is he still in practice?—Yes.

Where?—In Dublin.

Does he practise this particular inhaling method?—Entirely the inhaling method: that is, he has devoted himself exclusively to diseases of the chest. However, he will be able to give you his own views on the subject.

Have there been any later editions than the fifth edition of this book published?—I think there is a sixth edition.

When was that published?—I really do not know. It was some months ago, probably.

Re-examined by MR. COLERIDGE.

Is it in your knowledge that in your profession there are persons who devote themselves to special classes of disease?—Undoubtedly; it seems to be becoming a rule, in fact.

Most of the most eminent men in the profession do devote themselves to particular classes of disease?—They do.

As I understand you, with regard to this book, it does not profess to be addressed to the medical profession?—It is a popular treatise on the subject.

When you say it would be sufficient for medical men, could an ordinary instructed medical man, on reading this book, though not addressed to him, comprehend, and if he pleased apply the system which it defends?—Undoubtedly; he has sufficient to enable him to understand what is aimed at in the principles of treatment recommended, and to carry them out.

The Lord Chief Justice. How came those medical men to whom you have referred to come to you and ask you for information, if that was the case?—Not because they were unable to apply it, but because they wanted a little more detail than was conveyed in the work; the visits were probably as much friendly visits as anything else. We entered freely into conversation on the subject, and there was no effort to conceal anything from them.

Mr. Coleridge. Now I do not propose to take you through all these different passages at this time of night; but there are various passages in which you have described various symptoms as, in your judgment, showing that disease of the lungs exists where those exist; but may those symptoms exist and be referable in certain cases to other causes?—Undoubtedly; hence the importance of the examination to determine that.

Disease may exist in the lungs where none of those symptoms exist?—Yes.

When you are asked whether they are not symptomatic of disease of the lungs, do you lay any stress on the combination of symptoms?—Yes, we lay every stress on that.

There may be this or that symptom in a hundred other instances?—Yes.

But there is the combination?—There is the combination, but they must be taken together.

Cold hands and feet may be referable to other causes?—The habitual cold hands and feet, coupled with shortness of breath and loss of flesh, would be regarded by a competent authority as very suspicious indication, even where there was no cough and no expectoration.

Is what you recommend, when these symptoms arose, reference should be made to some one who can properly examine?—Yes.

The Lord Chief Justice. Surely all that is in the book?—All those passages that have been read are really little more than a description of disease that can be paralleled by almost every authority on the subject—they describe precisely the same symptoms in different language.

Mr. Coleridge. Now with regard to these various prefaces, it is intended to be imputed by my friend that they were written by you?—It is not true in any sense.

Mr. Coleridge. Then I will not ask you any more.

Adjourned till to-morrow at Ten o'clock.

THE INSTRUMENTS.

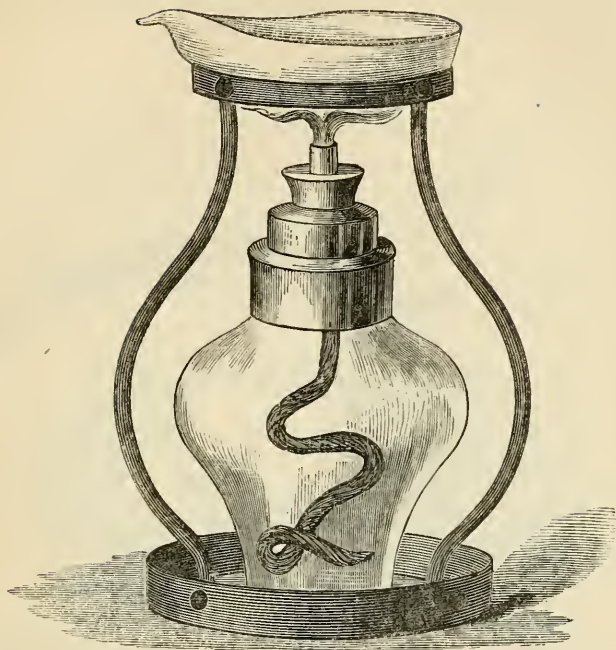
The several instruments employed by me in the treatment of affections of the throat and lungs, are, with the exception of the means of filling the vapour chamber, all described in my book as follows:—

THE CATARRH SYRINGE.

“Some years since, finding it impossible to effect a cure of these affections by such means, I contrived a *curved showering syringe*, by which I was able to make a direct application of any medicated wash to the *whole internal cavity of the nostrils at the same instant*. By daily cleansing the inflamed membrane, and strengthening it by astringent applications, I soon found that Catarrh was among our most curable affections. In the more aggravated cases of œzena and chronic Influenza I sometimes found the cure hastened by snuffing up certain vapours, which could not be applied equally well as a wash; and to meet this requirement I contrived a small copper sand-bath, to be used with a spirit-lamp. The medicines are placed upon a glass crystal and evaporated by heat. The patient holds his head over the bath, and receives the vapours as they come off into his nostrils. There is neither pain nor inconvenience in using these remedies. Even little children submit to them from day to day without complaint. By this simple, direct, and rational treatment, I have been able with certainty to break up these troublesome and dangerous affections where every other means had failed. When properly applied, the showering syringe and vapour bath will not fail in one case out of a thousand.”



THE VAPOUR BATH.

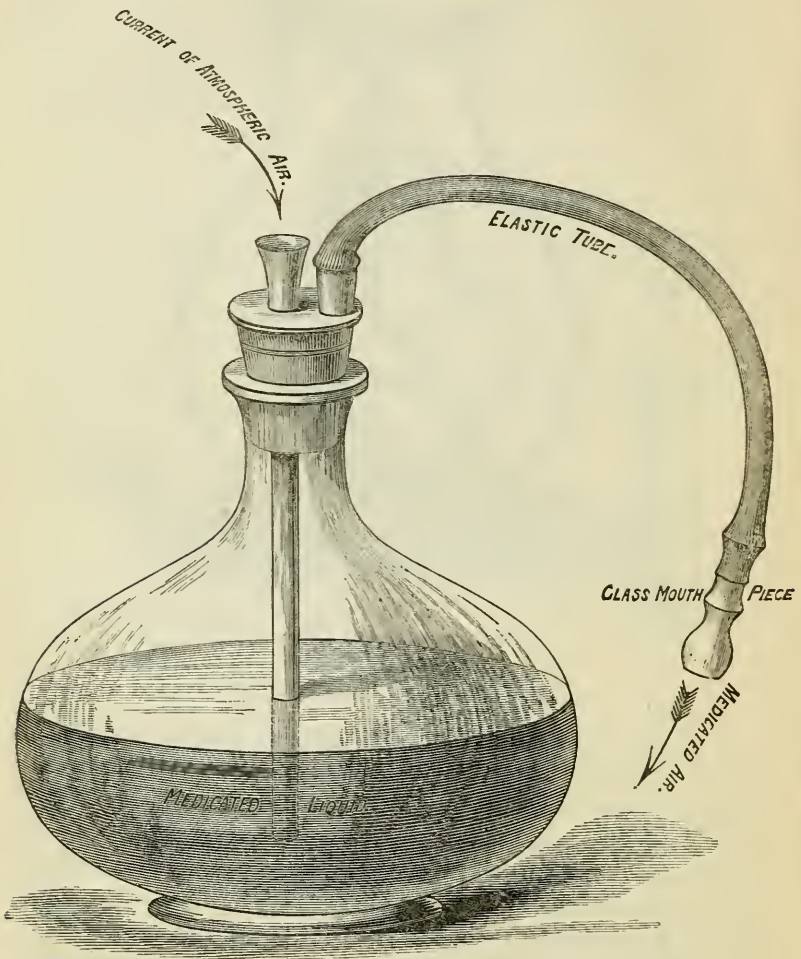


(Dr. Hunter's Letters, page 9.)

THE INHALING INSTRUMENT.

The following description of the inhaling instrument will be found on page 32, "Letters on the Lungs:"—"The inhaling instrument, which is made of glass, and holds about a pint of fluid, is half filled with cold, hot, or warm water, according to the nature of the case. The medicines prepared for a dose are then added, and the patient directed to inhale gently, but deeply, into the lungs; being careful to expand the chest well, without straining or violence. The fluid being medicated thoroughly impregnates the air with its properties. Each inhalation is continued for five, ten, or fifteen minutes, as may be necessary, and taken three times a day before meals, or twice a day on rising and

retiring. In this manner every effect that can be produced by medicine is *obtained in the lungs*, and with a degree of certainty unattained by any other method of treatment."



When to this is added the fact that I permitted their manufacture and sale, by the manufacturers of medical instruments for the benefit of the profession, I think it must be admitted that I

have treated them fairly—far more so than they were entitled to on any principle of reciprocity.

THE PASTILLE.

As the public has heard so much about the *mysterious* means employed by me of generating the “room vapour,” the following illustration of my pastille will, perhaps, be the means of enlightening them. I may say, in general terms, that it is a metallic cone, filled with whatever chemicals and drugs will produce the vapour I require, to accomplish any desired specific effect.



EVIDENCE OF PATIENTS.

EDWARD MOUNCEY, *sworn.*

Examined by Mr. COLERIDGE.

Do you reside near to Uxbridge?—I do.

And did you, in the month of August or the autumn of 1864, consult Dr. Hunter?—Yes, I did.

Was that for asthma?—Yes.

Had you been suffering from asthma for some time before you consulted him?—Yes, I had.

Had you been seriously inconvenienced by it?—So much so that I did not sleep at night without getting up to use some means of relief.

How long had you been under other doctors?—I had consulted other doctors, but I had not much confidence in them, and no expectation of their being able to relieve me.

And they did not do so?—I did not go through any course of treatment from other doctors.

How long were you under treatment by Dr. Hunter?—I should think six months.

Did you go on pursuing his system after that?—Occasionally.

How long altogether, at intervals, did you pursue Dr. Hunter's treatment?—I pursue it now if I catch cold.

You were regularly under his treatment for six months, and have used his apparatus from time to time since?—Yes.

Have you found any benefit from it?—Very great.

What was his treatment of you so far as you could judge? Did he make a careful examination and treat you carefully?—Yes; he stripped me, and examined me by the stethoscope.

And then he treated you after a careful examination?—Yes.

And you say you derived benefit from it? What was the scale of fees which you paid him—five guineas a month?—Yes, and one guinea first for the examination.

And that included attendance, instruments, and everything?—Yes.

Did you read his book?—Yes, I did.

You knew from his book who he was, and what his qualifications were?—Yes. I did not know that he was a Canadian. I imagined that he was an American doctor, but I did not take very particular notice.

Cross-examined by Mr. KARSLAKE.

Did you see his book in the form of an advertisement in the newspaper?—Yes, I did.

You found from that he could cure any one of asthma or bronchial affections?—No; I found out that he asserted he could cure people.

And that he could cure them, although the medical profession generally could not?—I had derived an impression, from my conversations with medical men, that asthma was a disease that was rather to be patiently borne and submitted to.

You had never submitted to any treatment for asthma?—Yes, I had; it was nothing of consequence.

You do not know what it was?—I have no idea.

Have you any idea what you took when you went to Dr. Hunter?—I have no doubt some inhalement of a very similar description to the one which has been described in the evidence this morning.

Have you any knowledge or notion yourself what you took?—I have none, except that I asked Dr. Hunter occasionally what it was, and he told me, though I cannot charge my memory with it.

Can you recollect what he told you it was?—Not at all, except that there was oxygen in everything.

You took oxygen for the asthma?—Yes.

He told you there was oxygen in everything you inhaled?—Yes, he did.

How much?—He did not tell me.

Or how it was produced?—No.

Lord Chief Justice Cockburn. You could hardly expect that he would do so.

Mr. Karlake. As far as you know, did you take stramonium?—I have no doubt I did.

Had you ever taken that before in the shape of pastilles?—No.

In what form, then?—Smoking it.

You went under Dr. Hunter's care in the autumn of 1864?—I did.

How long did you remain under his care?—I should think six months.

What was the charge—five guineas a month?—Yes.

Paid beforehand?—I paid him at the beginning of the month.

Had you more than one inhaler?—No, only one; but I have used others at Dr. Hunter's house.

How often did you inhale?—Three times a day.

Was that during the first part of the time?—Yes, during the first part of the time.

And after that, during the six months, did you diminish it to a considerable extent?—Not that I remember.

When did you give it up altogether; in what time; at the end of the six months did you give up inhaling—that was in the summer, in warm weather?—It is very likely; that would bring me through the winter to the summer.

You ceased to inhale during the summer?—Very likely not so regularly.

During the winter of 1865 did you take to inhaling again?—No, I do not think I did.

During the winter of 1865 you had not been inhaling?—Not regularly; I might occasionally, if I caught cold.

When did the six months' attendance cease?—If I began in September it would cease about March.

That was continuous?—That was continuous.

Do you consult Dr. Hunter now from time to time?—Occasionally. I can hardly say I consulted him; I did not want to do so.

Did you go and see him?—Yes; I like to have a supply of his medicines by me.

What are the terms now?—The terms are now very much left to myself.

It depends on how often you get fresh medicines from him?—Yes.

When was your attention first called to this?—It was perfectly accidental; I saw it in the *Times*.

Re-examined by Mr. COLERIDGE.

You have been to see other doctors in your life?—Yes.

Did they ever tell you what the prescriptions were, and how they were made up?—I have generally found that if I asked the question they would tell me; so I asked him, and he told me.

Major HUGHES, *sworn*.

Examined by Mr. COLERIDGE.

Are you a retired major in the army?—In the Indian army, on the Bengal establishment.

Had you been for something like ten years subject to bronchitis?—I have had it for ten years last month.

Had you consulted any medical men for your bronchitis?—Some of the most eminent men in London.

Did they do your bronchitis much good?—Not the slightest good.

And in the autumn of 1864 did you consult Dr. Hunter?—I first consulted Dr. Hunter on the 26th of September, 1864.

How did he treat you?—By inhalation—some kind of medicated vapour.

Was it exclusively inhalation, or did he also give you medicine through the mouth and stomach?—Occasionally, as I required them constitutionally, and other matters.

It was chiefly inhalation?—Yes, chiefly inhalation.

Did his mode of treatment improve your bronchitis?—Considerably. I will tell you its effect, if I am allowed.

That is just what I want to know?—In five days less than one

month after I had consulted Dr. Hunter on the 26th of September, 1864, I gained nine pounds in weight in less than one month.

Lord Chief Justice Cockburn. How as regarded the bronchitis?—The irritation was very much relieved, my lord.

Mr. Coleridge. After two months something more happened?—I gained seventeen pounds, with corresponding strength and health.

In another month you gained more?—At the termination of three months and eleven days I gained twenty-one pounds, which was the ultimatum of my increase.

During that time did the bronchial affection continue better?—Very much; considerably less. Before that I had been in the habit for eight or ten years of being almost choked at night with mucus, which mucus was streaked with blood.

What was the effect on the bronchitis and the mucus?—The secretions became much less, and the irritation in the throat considerably alleviated and palliated, and I had rest at night, some sleep undisturbed throughout the whole night, whereas formerly I was coughing all night.

Did your walking powers improve?—When I first consulted Dr. Hunter, I could scarcely walk half a mile to save my life; at the end of two months I used to take a four or five miles walk with great pleasure.

Has your cough or bronchitis remained better?—It has never been radically cured; there was always remaining a little hacking and irritation about the throat, but it was so reduced and palliated that it was really little or no inconvenience.

Does that better state of things still continue?—I caught a violent cold on the 9th of this month which threw me back again; it comes on again slightly.

Up to the 9th of the present month did it continue better?—Yes, previous to that.

You went in the autumn of 1864; from that time to the 9th of this present month did the bronchitis continue very much better?—Better and worse. I went a second time to Dr. Hunter.

When was that?—On the 20th of February this year.

Did he do you good then?—He relieved me, but not so much as in the first instance. It was wonderful in the first instance.

Are you still under him?—I placed myself under him again last Monday week for this fresh cold.

Are you getting better?—I was getting fast better in the course of a week—a great deal relieved.

What had you to pay him?—A guinea on the first consultation and five guineas a month afterwards, he being willing to supply me with medicines, instruments, and so on; and I was to call on him whenever I chose, and as often as I chose.

Did he examine you carefully on the first occasion?—Most carefully, with a stethoscope.

After he examined you he prescribed as you have told us?—Occasionally when he thought it was necessary.

Cross-examined by Mr. KARSLAKE.

Do I understand that for three months you were his regular patient?—I was.

Paying five guineas a month?—Yes.

After the end of three months did you leave him for a time?—No; I was ten months under him.

At five guineas a month?—Yes.

You went to him in September, 1864?—On the 20th of that month.

How long had you been home from India?—I left India twenty-two years ago.

Who was the last medical man whom you had consulted?—I should not think it right to mention names unless I was forced to do so; it would be a most invidious thing to do, because they did their utmost to cure me, and perhaps it was not their fault at all. It might be the fault of my own constitution; and to say anything before the Court regarding them would be most invidious.

Where were you living?—At No. 6, Princes-street, Hanover-square.

Had you consulted any medical man shortly before you went to Dr. Hunter?—For four, or five, or six years.

The same man?—No; several of the most eminent men in town on bronchial complaints.

When you went to Dr. Hunter were you very unwell indeed?—So unwell that I could scarcely walk.

Were you taking tonics during the time you were under him?—I took tonics occasionally, but the chief thing was the inhalation.

Was any particular diet ordered you?—Yes; I was to live well, eating plain food, and to drink two or three glasses of wine, no more—dry sherry.

And in three months you increased twenty-one pounds?—In three months and eleven days.

Has that fallen off since?—Yes.

When did it begin to do so?—Since I left in July; it commenced in February and left off in July.

The weight continued until the summer with little variation?—It did not continue the same to a pound or two; sometimes more, sometimes less.

Was it between the summer and the winter it fell off again?—Yes.

What is the reduction now?—I am less by about seven pounds than I was when I had increased twenty-one pounds.

Lord Chief Justice Cockburn. That is a clear gain of fourteen pounds from the commencement.

Mr. Karlake. How long have you been under his care since February?—I have been under his care until July of this year.

Did your weight increase in that time?—No; not so much.

You were not relieved so much on that occasion?—Not quite so much, but I was considerably relieved.

Not so much as at first?—No.

Was the treatment the same so far as you know?—Almost exactly the same so far as I am acquainted with it. I have no knowledge of medicines; I can only speak of their effect.

It had a soothing effect?—And was very pleasant to the taste also.

Were you taking cod-liver oil?—Before I went to Dr. Hunter I had taken it.

Since you went to Dr. Hunter?—No; one of the most eminent men in town told me it was poison, and I had better not take it, and he was quite surprised that I should have taken it for eight months and survived.

The last time you went was Monday week, and you are still under his care?—Yes.

Re-examined by Mr. COLERIDGE.

My friend says did you lose flesh in the summer?—was it in consequence of its being hot that you lost flesh?—No, I do not think that is so; but during the summer you dress more lightly than at other times.

Mr. Karlake. Your lordship will ask him when the weighing took place, and under what circumstances.

Lord Chief Justice Cockburn. Do you mean whether after dinner or before?

Mr. Karlake. No; the circumstances under which he ascertained that he had gained so much, or whether he was weighed with his great-coat on.

Lord Chief Justice Cockburn. Where were you weighed at this time?—At the Oriental Club, in Hanover-square.

Mr. GEORGE SEYMOUR, sworn.

Examined by Mr. COLERIDGE.

I believe you live in Arundel-gardens?—Yes.

Are you one of the firm of Seymour, Peacock, and Co.?—Yes.

Ship and insurance brokers in Fenchurch-street?—Yes.

Had you up to the autumn of 1864 been suffering from chronic asthma?—Yes.

In the course of the autumn of 1864 did some friend of yours mention Dr. Hunter's name to you, and did you consult him?—Yes.

For the reasons given by Major Hughes, I will not ask you their names, but had you before that time consulted several eminent medical men in London without success?—Yes, I consulted the late Dr. Addison, and was under his care for some time.

Did you derive much benefit from them?—No.

You went in the autumn of 1864 to Dr. Hunter?—Yes.

Did he examine you?—Most minutely.

With a stethoscope?—Yes.

Were you under his care, and did he afterwards prescribe for you?
—Yes.

For how long?—I consider that I have been under his care ever since.

Has he done you good?—Yes, a very great deal of good.

When you say chronic asthma, was it asthma of many years' standing?—Just seventeen years'; since November, 1849.

What was the treatment?—By inhalation and medicine.

Partly one, partly the other?—Yes.

You say that the success was marked?—Quite so.

Had you reason to be satisfied, so far as you could judge, with all that Dr. Hunter did for you?—Perfectly satisfied.

And with Dr. Hunter himself?—Perfectly.

And with his charges?—Perfectly.

You have mentioned chronic asthma; besides that, during 1864 you had an acute attack?—I have had several acute attacks, which Dr. Hunter has treated me for.

For what?—For inflammation of the lungs.

And treated you successfully?—Yes; not by inhalation. At times, in the interval, he did occasionally apply inhalation; generally, the treatment was something similar to the homœopathic doctors'.

Lord Chief Justice Cockburn. That is when the inflammation was acute?—Yes.

Mr. Coleridge. The treatment varied according to the disease?—Just so.

Have you had in your house relations of your own whom he has also treated?—Yes.

I think a nephew of yours?—Yes; he was most successful in that case.

Lord Chief Justice Cockburn. What was the matter with your nephew?—General debility, and an affection of the mucous membrane of the head; a stoppage in the head, and his hearing affected. That was eighteen months since. He made a perfect cure of him, and the lad has grown stout and strong.

Was inhalation resorted to there?—Yes, and some other things I am not acquainted with—probing the nostrils.

The main affection was an affection of the nostrils?—Yes, as the doctor described it to me, the mucous membrane of the head.

And he cured him, did he?—It was a perfect cure.

Mr. Coleridge. Was there also a young lady in your house?—Yes.

A relation of yours?—A relation of my wife.

Was she also very much benefited?—She was; she was only a short time under his treatment; probably two months.

Lord Chief Justice Cockburn. What was that for?—For asthma; she has been much better since.

Cross-examined by Mr. KARSLAKE.

When was she with him?—Probably fifteen or sixteen months since.

Did the asthma come on when she was at your house?—No; she had suffered from asthma for many years before; from a child.

She got better under his treatment?—Yes.

The lad you spoke of, your nephew, he suffered from general debility?—He was a badly grown boy. I sent him abroad, to South America, for his hearing, and he got better from the voyage; he was away sixteen months. On his return I sent him to school in France, and the deafness came back to him; he came back quite deaf; then I sent him to Dr. Hunter, and the result was what I have stated.

You say there was a probing of the nostrils from time to time; were they stopped?—You could observe by his speech there was a stoppage of the nostrils.

Did you attend at that time?—I have seen Dr. Hunter treating him.

Did he inhale through the nostrils?—Yes.

Lord Chief Justice Cockburn. Was the probing to ascertain the state of the nostrils?—I should think it was to open the passage.

Mr. Karslake. When you had this acute affection of the lungs you were not treated by inhaling, but by the homœopathic treatment?—Yes.

Had you been under homœopathic doctors before?—Yes.

For some years?—I tried it for some months.

What was the treatment of Dr. Hunter other than inhalation?—Certain draughts, and matters in that way, which I do not understand.

Through the stomach?—Through the stomach.

With blisters?—No.

Embrocations?—Yes; he ordered blisters, but I objected to them.

You had no blisters; you had embrocations?—Yes.

The inhalation was only for the chronic asthma?—As I imagined more especially for the chronic disease.

Were you taking medicines besides the inhaling?—After the inhalation there was generally a small dose of what he called alterative; a spoonful, mixed with water, which I took after each inhalation.

Mr. Coleridge. You were some months under homœopathists who did you no good?—I was nine months under the celebrated Dr. Cary, who did me no good.

Colonel FELIX THURBURN, *sworn.**Examined by Mr. COLERIDGE.*

I believe you are a colonel in the Indian army?—I was a lieutenant-colonel.

Have you for many years suffered from inflammation of the mucous membrane?—I had a sort of chronic catarrh.

How long had that been so?—The irritation had continued for several years.

And had you consulted medical men about it?—I had been more or less under treatment for it for some years.

But without success?—Without success.

Did you in the autumn of 1864 consult Dr. Hunter?—Yes, in the autumn or the winter; I am not quite sure.

The latter part of 1864?—Yes.

Did he examine you?—He examined the nostrils and my throat.

Did he treat you for the irritation?—Certainly.

Did you learn from him what hope of cure there was?—He told me it would be a very tedious cure—take a very long time.

Did he do you good?—He did during the time I was under him.

Lord Chief Justice Cockburn. How long were you under him?—About three months, I think; I had occasion to go away from London, and I left him.

He did you good during those three months?—Certainly.

Mr. Coleridge. Has the irritation and discomfort been less since that?—I have not had so much headache as I used to have. I used to have headache accompanying it. The irritation still remains.

Lord Chief Justice Cockburn. You have not been under him now for how long?—For eighteen months.

Mr. Coleridge. The irritation continues, but you have not so much headache?—The headache is much improved.

Does the improvement of the headache date from the time of your consulting him?—I think so.

He told you it would be a tedious thing?—He told me at the time of the first or second examination that it would take a very long time to cure.

Did he say there was a particular portion he could not get at?—He told me there was a cavity he could not reach hereabouts, between the eyes.

About the same time that you were consulting him, did Mrs. Thurburn consult him?—She did, a little previous to my going under his treatment—a month or so.

Did he do her good?—Very much so; she benefited very much.

What was the matter with her?—It seemed to me to be general debility; it was more a bronchial or lung complaint.

Did she get better under his treatment?—She got stouter, certainly, and much better.

During the time that Mrs. Thurburn was under him and you were under him, you had opportunities of observing him—were you satisfied with his treatment?—Very much so; he was very attentive to us both.

Were you yourself satisfied with him? Did he appear to you, so far as you could judge, to be a competent person?—Very much so; when we left his treatment, he told us that any time we liked to come he would take up the case without any further charge.

Were you satisfied with his charges?—They were very moderate.

Cross-examined by Mr. KARSLAKE.

They were the usual charges—one guinea in the first instance, and five guineas a month?—Yes.

You were with him in the first instance?—Mrs. Thurburn was with him in the first instance.

How long was she with him?—I should fancy about four months.

At the regular charge?—Yes.

How long were you under him?—Speaking from memory, I was three months.

You told my friend you had been under medical treatment before?—Yes, I came home under medical treatment.

How long before?—In 1862.

The first time you suffered this inconvenience was in India?—Yes.

How long ago?—Twelve or fifteen years ago.

It had been going on more or less ever since?—Yes.

Had you consulted medical men in India specially for it?—I had been under the treatment of several medical officers in India.

Had you consulted any one in London before Dr. Hunter?—Not since I came home in 1862. Previously I did when on furlough.

Did you use inhalation?—No, I used a pastille. You may call it inhaling.

Through the mouth or the nose?—Through the nose.

And medicine as well?—Alterative medicine.

Mr. HENRY REEVES, *sworn.*

Examined by Mr. COLERIDGE.

I believe you live at Winterborne in Wiltshire, near Swindon?—Yes.

Have you a large farm there?—I have.

For some fifteen or sixteen years before you consulted Dr. Hunter, had you been suffering from what is called dry asthma?—I had.

Had you consulted several medical men for it?—I had,

With any success?—None.

Had you changed your place of abode from time to time, and tried the south of England, the Channel Islands, and other places, in order to get any benefit if you could?—I had.

Did you get any?—I derived great benefit some few years previous to consulting Dr. Hunter, but as soon as I returned to my native county I felt bad again.

You got benefit by change of air?—Not the same year that I consulted Dr. Hunter. I found no benefit from travelling.

As soon as you got home you got ill again?—Yes.

In the autumn of 1864 had you fallen away a good deal in weight?—Very much.

In 1864 did you consult Dr. Hunter?—I did.

Was it about the 15th or 16th of November?—It was the early part of November.

Did he examine you?—He did very minutely.

Did he, after examination, proceed to treat you?—He did from that day.

Did you remain in London for some time to be under his immediate superintendence?—Nearly a month.

At the end of that month had you got better?—Very much better, so much so that I was able to return home. When I first consulted Dr. Hunter, he said I should return to a warmer climate and remain there for the winter.

Dr. Hunter told you that?—Yes.

You got better and was able to go home?—I was able to go home for all the winter.

Lord Chief Justice Cockburn. At the end of the month you were materially better?—I was very much better indeed.

Mr. Coleridge. Did you continue Dr. Hunter's method of treatment after you got home?—For three months after.

How did you manage it—by correspondence, or how?—I saw Dr. Hunter once a month. I went up and saw him.

Then he gave you instructions for the intermediate time?—And a fresh supply of medicine.

Did you improve in weight?—I did, very much.

Did you get back more than you had fallen away?—I believe I did.

Did you continue that treatment beyond the three months, or did you stop then?—At the end of three months, when I left London—four months after I had commenced the treatment—Dr. Hunter told me I need not continue it longer. I had better take a month's supply in case of a relapse. I have that by me. I have had no relapse.

Lord Chief Justice Cockburn. You have been quite well ever since?—I have been quite well ever since.

Mr. Coleridge. Were you perfectly satisfied with the way Dr. Hunter treated you?—Very much so. I never received so much consideration from any other medical man.

Were his charges what we have heard?—Precisely the same.

Cross-examined by Mr. KARSLAKE.

A guinea in the first instance, and five guineas a month afterwards?—Yes.

You saw Dr. Hunter when you came from the country once a month?—Yes.

And paid the five guineas in advance?—I paid the five guineas and had the supply for a month.

What had you besides the inhaling?—I had the inhaling, the alterative medicine, an embrocation to rub into the chest, and a pastille to burn in my room at night.

Did you continue using the alterative and embrocation from time to time?—I did.

Throughout the whole of the treatment?—I did the alterative. I think I left off the embrocation after some time.

Did you continue the inhaling?—Yes. All the four months. I never missed once.

Was it the same during the four months?—No; it was changed from time to time.

That you judged of by the taste, I suppose?—Yes, and Dr. Hunter told me he was changing it.

You had lost weight before you consulted Dr. Hunter?—I had lost weight so very much that, in fact, I could scarcely walk with a stick before I went to Dr. Hunter.

When was that?—In the autumn of 1864.

When did you recover your weight?—Within three or four months.

What was your weight when you consulted Dr. Hunter? Did you put it down?—I think I weighed ten stone two previously; when I went to Dr. Hunter I weighed nine stone four as near as I can recollect.

At the end of six months?—I regained my weight, and I have kept it ever since.

Lord Chief Justice Cockburn. You got back to your old weight?—Just so.

Mr. Karlake. You were there in November, December, and January?—November, December, January, February, and March.

And one month in London?—Yes.

Had you ever stayed a month in London before?—No.

Mr. Coleridge. The effect of that month in London has remained ever since?—It has.

Mr. THOMAS DUNN, *sworn.*

Examined by Mr. COLERIDGE.

You live at Reigate?—Yes.

Have you been the mayor of that place?—I was the first mayor of Reigate, and am still an alderman.

Have you many years suffered from asthma?—Yes.

Before you consulted Dr. Hunter had you consulted many other medical men?—I had spoken to my own medical man, who had not paid any great attention to it, and considering it was a disorder utterly incurable, I did not trouble myself about it. I felt it was useless to go to any medical man.

You did not consult any one else?—No; I felt it was incurable.

Was it painful and inconvenient to you?—Very.

In the present year you consulted Dr. Hunter?—Yes, in May.

Has he done you good?—A great deal; so much so that I feel it almost a duty to recommend any of my friends attacked with asthma

to consult him, and I have recommended several who have derived great benefit.

Has your life been more comfortable?—Very much more.

Are you well satisfied with him?—Very well satisfied indeed.

Did he treat you with inhalation?—Yes.

And other medicines besides?—Yes.

Were his charges what we have heard of?—Exactly the same.

Cross-examined by Mr. KARSLAKE.

You made up your mind asthma was incurable, and took little trouble about it?—I thought it was generally acknowledged that asthma was incurable. I was induced to go to Dr. Hunter from the recommendation of a friend who had received so much benefit that he urged me to go, and at last I was induced to go.

In May of this year?—Yes.

Did you inhale for months or weeks?—I kept it on from May regularly until within a week or two, when I was so much better that Dr. Hunter considered that I might leave off inhaling three times a day, and do so once, which I am now doing.

Have you been taking other medicines?—Other medicines Dr. Hunter has given me.

You have been exclusively under his care?—Entirely.

Were the inhalations altered from time to time?—Not that I am aware of.

You did not discover any difference in the taste?—Not any particular difference; there may have been; I did not notice it.

Was yours a dry asthma?—I believe it was.

Mr. GEORGE ROBINSON, *sworn.*

Examined by Mr. COLERIDGE.

I believe you reside at Chalfont St. Giles?—Yes.

You have a farm there?—I rent one.

Some years before 1864 had you suffered a good deal from bronchitis?—Yes.

Had you tried different doctors?—I had been under different local doctors.

They had not done you any good?—They did not give me permanent relief.

In the autumn of 1864 were you again suffering from bronchitis?—Yes.

Was that a very bad attack?—Yes, it was a serious attack.

Had you become very weak?—I was very weak indeed.

Did you feel altogether ill?—Yes.

In November, 1864, did you consult Dr. Hunter?—I did,

Did he do you good?—Yes, most undoubtedly.

Did you spit blood at that time?—There were streaks of blood in the phlegm.

How long were you under his treatment?—At the end of four months I gradually discontinued it.

Lord Chief Justice Cockburn. You remained under his treatment four months?—Yes; at the end of four months I gradually left it off. I continued the inhalation but once a day or so; it was gradually left off, not suddenly.

Have you been better ever since?—Yes; I have derived permanent benefit from it.

You have since had a cough, for which you again took Dr. Hunter's remedy?—Yes, I had a little of his attention last winter. Dr. Hunter led me to expect I might want a little attention last winter.

You were attended for two or three weeks?—Yes.

Did you get better?—Yes.

Have you been better ever since?—Yes.

Have you been satisfied with what Dr. Hunter has done for you?—Yes, very much so.

And with the charges he made?—Yes.

Cross-examined by Mr. KARSLAKE.

You knew there was a regular scale of charges?—Dr. Hunter informed me so the first time I saw him.

What did you understand you were suffering from?—Bronchitis, with tubercular deposit.

Did you take medicine as well as inhale?—After the inhalation I took a small quantity of some medicine.

During the winter of 1864 did you inhale frequently?—Three times a day, and a pastille when I went to bed.

How often did you see Dr. Hunter?—Once a fortnight.

For the first three months?—All through the four months.

Were you with him four months?—Yes, I think so.

During the summer you were better?—Yes, I was better—especially in the latter end of the summer.

I suppose you always felt better in summer than during the cold weather?—I have suffered from bronchitis in the summer time.

Generally you were better in the summer?—Yes; I generally had it most in cold weather.

When did you go again from the cough?—Last winter—about a week after Christmas.

Have you been to him lately?—No, not since then.

Are you inhaling now?—No.

Do you suffer now from bronchitis?—Not at all. I may have an occasional cough—I am not suffering from it.

You consider yourself cured?—I do—comparatively so. I do not intend to tell the Court that I never cough—I consider myself comparatively cured.

You went on a second occasion—did you take medicine as well as inhaling?—I think I took some medicine—I am not certain. I had a cough-mixture. I was to use as little of it as possible.

Lord Chief Justice Cockburn. Comparing the state you are in with the state you were in when you first went, do you think your money has been thrown away?—Not at all, my lord.

Mr. EDWARD CONDER, *sworn.*

Examined by Mr. COLERIDGE.

I believe you carry on business at the Baltic Wharf, Kingsland-road?—Yes.

You consulted Dr. Hunter early in 1865?—I did.

Had you been suffering for some years before from asthma?—About three years.

Had you consulted other persons before?—Several of the best physicians in London.

Had they cured you?—No.

Did Dr. Hunter examine you?—Yes.

With a stethoscope?—Yes.

Did he treat you for three months?—Yes.

At the end of that time were you better?—Very much better—I was not cured.

But you were very much better?—Yes; occasionally I had a relapse—very little.

When you had a relapse?—I applied the inhaling.

Did that do you good?—Yes, directly—in a few days.

Comparing your state before you saw Dr. Hunter with that you are in now, do you think you are permanently better?—I do. If I catch a little cold by walking too fast, I may have a relapse; but it is only for a day or two, and then I apply the inhaling. It is very seldom I have it now; before I had it once every week.

Are you satisfied with what Dr. Hunter has done for you?—Quite satisfied.

And with the charges he made?—Yes.

Lord Chief Justice Cockburn. A man has a right to make his own terms.

Mr. Coleridge. It has been said he was extorting enormous fees.

Cross-examined by Mr. KARSLAKE.

When you get a relapse you use the inhalation?—Yes.

And the medicine?—No—nothing but the inhaling.

Had you not medicine as well?—I had an alternative.

How long had you suffered from this?—About three years.

Lord Chief Justice Cockburn. Did it cause you much inconvenience?—I was often obliged to sit up all night. I could not lie down—when I laid down I fancied I was going to die. I could not breathe.

That is entirely relieved?—Yes, except when I have a little relapse.

Except when a little relapse comes on, you have been entirely relieved from that?—Quite so.

Mr. JAMES NORE LEE, *sworn*.

Examined by Mr. COLERIDGE.

You are one of the editors of *Bell's Weekly Messenger*?—Yes.

Before consulting Dr. Hunter had you for many years suffered from asthma?—About twelve years.

Had you consulted any other medical man?—I had.

With any success?—Slight success for a time; no permanent advantage.

In October, 1864, did you consult Dr. Hunter?—In September or October.

And did he examine you with a stethoscope?—Yes.

How long were you under his treatment?—I was under his treatment two months; besides the asthma I laboured under a polypus—both the nostrils were filled. The last physician whom I consulted, when I called his particular attention to that, told me it was not a matter of much consequence; but under Dr. Hunter's treatment I was relieved from that; it was cut out. I had not been under his treatment more than three weeks before the cough, which had previously been of a very distressing character, with copious and very foul expectoration, left me, and in two months my cough was greatly relieved.

Lord Chief Justice Cockburn. You had bad expectoration?—Yes, very foul expectoration, frequently streaked with blood. At the end of two months I felt myself so well that I did not think it needful to go to Dr. Hunter any more.

You felt yourself quite well, and discontinued your attendance on Dr. Hunter?—Yes. In the course of the summer, I found the polypus was growing again. I had taken cold, and a difficulty of breathing came on. I consulted Dr. Hunter, and underwent another examination. He treated my nose as he had done before. I had hoped he had effectually removed the polypus; but I am rather doubtful on that point. I think a little of it is still remaining; but as far as my general health is concerned, I can only say that I am a perfectly different man to what I was before I went to Dr. Hunter.

Mr. Coleridge. I need hardly ask you are you perfectly satisfied with what he has done?—Perfectly satisfied.

And with his charges?—Yes, most decidedly.

Cross-examined by Mr. KARSLAKE.

When did you first feel the bad effects of this polypus?—I was not aware what it was.

Until Dr. Hunter told you?—I had felt the effect of it. I could not breathe through my nose but with great difficulty.

Was that the painful sensation under which you were labouring when you went there?—I suffered from great difficulty of breathing, and a very severe cough and expectoration.

Did Dr. Hunter operate on you for polypus?—Yes.

You felt better as soon as it was removed?—I felt better before it was removed.

What had been the course of treatment?—What you have heard described by several gentlemen—the inhaling through the mouth, and using the pastille at night.

And taking any medicines?—Only the alterative after inhaling; nothing more.

Was your complaint asthma?—I concluded it was; I had great difficulty in breathing, and copious foul expectoration.

Lord Chief Justice Cockburn. If there were any obstruction in the nostrils by polypus there would be a difficulty of breathing through the nostrils. Had you also difficulty in breathing through the throat?—Yes.

Mr. Karlake. How long had you experienced a difficulty in breathing through the throat?—I had experienced it for years.

How long had the stoppage in the nostrils been going on?—For a number of years.

It got very bad before you went to Dr. Hunter?—Yes.

Lord Chief Justice Cockburn. When did he remove the polypus?—I think in the course of the first two months.

He removed that surgically?—Yes.

Do you remember how long you had been under his treatment before that operation?—He commenced to perform the operation the second time I saw him.

It was not done at once?—No, it was a very tedious affair.

Mr. JAMES EIVES, *sworn.*

Examined by Mr. COLERIDGE.

You carry on business at 61, King William-street?—Yes.

And are in the American trade?—Yes.

Did you know Dr. Hunter in America?—No, not till he came to England.

Before the autumn of 1864 had you been suffering from a bad malignant catarrh?—Yes.

For a long time?—Yes.

Had you consulted persons about it?—I had.

Could you get any relief?—Some.

Not permanent or perfect?—No.

In October, 1864, did you consult Dr. Hunter?—I did.

Did he examine you?—Yes.

After that did he treat you by inhalation?—Yes.

And did you become better?—Much better.

Was it an improvement that continued?—Quite so.

Have you since been from time to time under his treatment?—For three or four months; from November to March.

Has that produced a considerable improvement in your health?—Quite so; I have had recourse to no other doctor.

Has he done you good and improved your health?—Most entirely; I have been well since.

Lord Chief Justice Cockburn. You have had no return of this?—No return.

Mr. Coleridge. Has some member of your family also been under him?—Yes, my daughter.

Has she also benefited by his treatment?—Very greatly.

Was hers some throat or chest attack?—It was a chest attack.

And she benefited considerably?—She did greatly.

Have you been satisfied with what Dr. Hunter has done?—Quite so.

And with what you have had to pay for it?—Yes.

You think you got money's worth for your money?—I do.

Cross-examined by Mr. QUAIN.

How long had you had this bad catarrh?—More or less for some three or four years.

Can you describe the nature of it?—It was a polyphoid affection of the nose.

Was there a discharge?—At times a very considerable discharge.

How long were you under Dr. Hunter's treatment?—From November to March.

Did you go every week during that time?—In the earlier part I did.

Lord Chief Justice Cockburn. Was there any surgical operation?—I had had them.

Not under Dr. Hunter?—No; not under Dr. Hunter.

Mr. Quain. Was any probe used by Dr. Hunter?—I can hardly remember.

You would recollect a probe being put up your nose?—I have had that done by different medical men.

Not by Dr. Hunter?—I cannot remember whether he did.

When did you last consult him?—I think in March, 1865.

Have you continued the treatment since then?—I have had no necessity to do so.

Did you inhale through the nose?—Through the nose entirely.

Did you do so at your own house?—I inhaled entirely at my own house; he injected at his house.

Mr. CHARLES BUSS, sworn.

Examined by Mr. COLERIDGE.

You carry on business on Holborn-hill?—I do.

Have you been nearly all your life subject to spasmodic asthma?—I have.

Have you suffered from it severely, so as to interfere with the comfortable discharge of your business?—I have for months together.

Have you consulted doctors for it?—I have been under several very eminent physicians.

Did they cure you?—They gave me some relief, but where we were at issue was, that they administered chloride ether, which so affected my stomach that I rebelled against it.

Did the stomach or you rebel?—My stomach rebelled against the medicine. After taking this I found that I could not eat anything. I was advised to take nourishing things, but the medicine produced such a sickness that I could not eat anything.

Your stomach objected to the medicine?—Yes. In October, 1864, seeing Dr. Hunter's letter published in the *Times*, and having a predisposition to inhalation, I was induced to go to him to see if I could escape from the ether.

You consulted Dr. Hunter?—I placed myself under his care. Having been examined by him, he told me my lungs were very sensitive and tender, which I knew well before.

Did he do you any good?—Yes, he was very successful indeed; so much that after I had been under him a month I felt myself so much better that I declared off his books. I should tell you that Dr. Hunter was in my case very liberal—he may be with others—but he gave me a supply of medicines which lasted me considerably longer than my month, for which I paid six guineas. He had so many patients I was obliged to wait for my turn, and I told him for the future I should write to him and have his medicines sent to me, which I have done; and I may say with truth, had I not had the benefit of his medicine I could not have ventured out in the heavy fog.

Are you the better for it?—Much better.

He has done you real good?—Yes; he has given me the inhalation instead of this abominable ether, which I scarcely ever think of without vomiting.

Are you satisfied with what he did for you, and what you have paid?—Perfectly satisfied. The last case of medicines he sent me, for which I paid five guineas, has lasted me with economy, and I am very careful in all my habits.

Up to the present time?—It has lasted me three months, which is about 8s. 9d. per week, which I think is not very extravagant for a doctor's bill.

Cross-examined by Mr. KARSLAKE.

I will only trouble you with one question. Where do you live in Holborn?—No. 34, Holborn-hill.

And what are you?—A tailor.

Mr. HENRY CAWTHRA, *sworn.*

Examined by Mr. COLERIDGE.

I believe you are a Canadian barrister?—Yes.

When at home living at Toronto?—Yes.

Your family of the last generation lived in England?—Yes, on both sides.

Before your father and mother emigrated, did they know Dr. Hunter's father in England?—I do not know about that.

In Canada you knew Dr. Hunter and his family?—Yes.

How long have you known him?—I have known him since I was a boy.

Was he the family doctor of your house?—Yes, he was.

And have you been under Dr. Hunter?—Yes, I have.

What was the matter with you?—I had chronic catarrh, or an affection of the nostrils, almost ever since I can remember—from boyhood.

Was it so as to be unpleasant?—Yes, my nostrils were so stopped up that sometimes I was almost suffocated when eating. I had great difficulty in eating; it necessitated breathing through the mouth while eating.

Had you consulted medical men in Canada?—I had, without success.

Did you go to New York where he was practising?—Yes, I did.

Did he examine you?—Yes; he examined my chest and pronounced it perfectly sound. He injected different things into the nostrils by a little instrument which he put into the mouth and injected it through the nostrils instead of running down the throat as other doctors had made it do; it came through the nostrils and gave me instant relief. I pursued that practice with him for some time.

Did he effect a cure of you?—I think I may attribute the cure to his treatment entirely.

Have you been better ever since you consulted him? Yes, it has lost its chronic state. If I have a cold it returns a little.

Lord Chief Justice Cockburn. Just while the cold lasts?—Yes. Dr. Hunter gave me a prescription, which I use when I have a severe cold.

Mr. Coleridge. The fees in New York are regulated by statute?—I do not know anything of the law of New York; I am a Canadian.

What did you pay him?—I understand the fees were twenty-five dollars. When I spoke on the subject of remuneration, Dr. Hunter would not hear of it; but I gave twenty dollars to his agent before I left.

He also treated your mother for asthma?—My mother-in-law.

Was she subject to asthma?—Yes.

Did she come over to England?—She came over with me.

Was she treated by him while she was here?—No. She sent over from Jersey for some pastilles.

Cross-examined by Mr. KARSLAKE.

In what year did you consult Dr. Hunter?—I think it was in 1854.

Then you were residing where?—At Toronto. I went to New York to see him.

You were suffering from cold?—I suppose it was the result of frequent colds.

I want to know what you mean by catarrh?—A distressing feeling in the nostrils and immediately behind the palate. I do not know the medical term.

Was that removed by means of the syringe you spoke of?—I used them both together. The syringe did most service; but I used to derive great solace from an inhalation he gave me through the nostrils.

Has that unpleasant feeling continued?—No; my nostrils are as free now as I could wish.

Mr. HORATIO NELSON HORNBY, *sworn*.

Examined by Mr. COLERIDGE.

You live near Swindon, and are chief clerk in the locomotive department of the Great Western Railway?—I am.

In 1864 had you a bad attack of bronchitis, which settled on your lungs?—Yes, upon one lung.

Had you consulted the local medical men there without much benefit?—Yes, I had.

From seeing one of Dr. Hunter's books you consulted him later in the year?—In September of that year.

Did he examine you very carefully, and did he treat you from that time up to Christmas?—Yes; from September 23rd to Christmas.

Did you get benefit from his treatment?—Yes, very great; I was very weak indeed when I went to Dr. Hunter, and I believe I had the usual remedies, which did me a great deal of good—inhalation, embrocations, and pastilles.

By Christmas, 1864, were you much stronger than when you consulted him?—Yes, very much stronger.

Have you remained better to the present time?—My general health has been much better.

Lord Chief Justice Cockburn. You got rid of the affection of the lungs?—Yes.

Mr. Coleridge. And you have remained better ever since?—I have had colds since then.

Then have you had recourse to Dr. Hunter's remedy?—Yes, always.

And with success?—Very great success.

Are you satisfied with what he has done for you?—Yes, very much so.

And with what you had to pay for it?—Yes.

Cross-examined by Mr. KARSLAKE.

You say you had bronchitis which settled on one lung; what were the symptoms?—Shortness of breathing, profuse perspiration on the least exertion, and great weakness, coughing, and expectoration.

Did that come on suddenly?—Yes, it did; I took a violent cold.

How long did the violent cold continue before those aggravated symptoms set in?—I think not more than three or four days.

Was that the first time you had those symptoms?—Yes; I never had been told by any medical man before that my lungs were affected.

Dr. Hunter told you that all these things were due to the state of the lungs?—Dr. Hunter told me so, and also other professional men previous to that.

Lord Chief Justice Cockburn. What professional man had told you so?—The country practitioner at Swindon.

What did he tell you?—He told me my lungs were affected from the effects of bronchitis.

Mr. Karlake. How long before you went to Dr. Hunter had you consulted the gentleman at Swindon?—I suppose I was under the gentleman at Swindon about three months. I then consulted a physician in London, under whom I was six weeks. He also told me my lungs were affected slightly.

Was it inflammation of the lungs, or what?—That I cannot tell you.

This disease of the lungs had been in progress four or five months before you saw Dr. Hunter?—Yes.

Lord Chief Justice Cockburn. What were the symptoms under which you were labouring when you saw Dr. Hunter?—Shortness of breath, great weakness, causing a great deal of coughing in the morning, with expectoration slightly streaked with blood.

And what about the perspiration?—A profuse perspiration on the slightest exertion. I could not walk up-hill at all.

Mr. Karlake. A good deal of irritation in the throat?—Yes, a very great deal.

And coughing?—Yes. I also suffered very much from ulcerated sore throat.

You had done so before?—Yes.

On several occasions?—Yes.

Was that accompanied with cough?—Yes.

Was there anything else?—I had generally a severe pain in my chest, on the left side. I was told my right lung was affected, for which I had been prescribed iodine.

Who had prescribed that?—I did not mention the name.

Did those symptoms continue when you consulted Dr. Hunter?—Yes.

Lord Chief Justice Cockburn. You mean the pain in the chest or the back?—In the chest.

Mr. Karlake. Had the pain in the back ceased?—I still had a pain there.

Did you stay in town when you consulted Dr. Hunter?—I came backwards and forwards from Swindon once a week regularly for two months.

Your treatment by him was inhaling?—Yes.

Any medicines?—A tonic or alterative after each inhalation.

Anything else?—A pastille at night, a powerful embrocation, cough mixture, and a gargle.

How soon after consulting Dr. Hunter did you derive any benefit?—Within a fortnight.

The pain had decreased?—Yes; my chest was relieved by the inhalation, and I could expectorate freely without exertion.

Lord Chief Justice Cockburn. How soon was that?—Within a fortnight.

Mr. Karlake. The cough was loosened and the expectoration more easy?—Yes.

Did the throat get better?—Yes, after having a part of the uvula removed.

Did Dr. Hunter do that?—Yes.

That was elongated?—Yes.

How soon after was it that the pain in the back and the chest went away?—I do not think I could rightly remember that.

In the course of three months did it entirely go?—Yes.

And your breath returned?—Yes, I am rather short now.

Were you always rather short of breath?—No; I presume that is from the lung being dried up.

There is no pain there?—No.

The cough is loosened?—I do not cough except occasionally.

Did you cease to consult Dr. Hunter at Christmas, 1864?—Yes, as to consulting him continually. I am never without his medicines now.

You still continue to inhale?—Yes.

And have the pastille burned?—Yes, if there is necessity for it—if I take a violent cold, which I am rather subject to. I am exposed very much.

Do you still use the alterative?—Yes, after inhaling.

And the treatment as before?—Yes.

Mr. Coleridge. This is the plaintiff's case.

Leon Contanseau.—I have been living at Saint Alban's-road, Norwood, but am now staying at No. 13, Park-road-terrace, Forest Hill. I lately held the office of professor of French at Addiscombe College, and Government examiner for candidates for commissions. I know Dr. Hunter, and have consulted him professionally. I was suffering from palpitation of the heart, and from debility to such an extent that I could hardly walk any distance without being out of breath. I also broke into sweats, and had tightness on the chest, with expectoration, not much, but which I did not like. I had a little hacking constant in the throat, which I can hardly call a cough. I had suffered four or five months before I went to Dr.

Hunter. I had occasionally, during that period, consulted other medical gentlemen before I went to Dr. Hunter. I went to him first at the end of November, 1864. I saw him at his own house. I told him what I was suffering from. I stripped, and he examined my chest very minutely. He told me that the upper lobe of my lungs was slightly attacked, and that it was quite time to begin treatment. He gave me an apparatus: a water-bottle, with a gutta-percha tube at the top, and I had to inhale three times a day—in the morning, the middle of the day, and the evening. He also gave me a tonic to be taken each time after inhalation. He said that he hoped in a month or two I might be cured. The very first week in which I began inhalation I felt very great relief. I was to inhale fifteen minutes at each inhalation. He told me to eat as much meat as I could. I had no appetite and could not eat then. He ordered me French light wine. He told me never to be without flannel, and to take great care not to catch cold or get wet. I followed out his directions punctually, and became so well, that I have since been able to attend to my duties without difficulty. I gained from eighteen to twenty-one pounds weight in three months. I first began to feel my palpitation disappear little by little; then I recovered my sleep, which I had previously lost. I recovered also my appetite; the sweats disappeared; pain in my legs, from which I had suffered, disappeared; so also did the tightness in the chest. The hacking in my throat went off too. I continued the treatment, notwithstanding my improvement. I left it off at the end of three months afterwards, using the inhalation occasionally when I did not feel quite so well. Dr. Hunter made many examinations of me. He charged me one guinea for the first visit, and five guineas for the first month, he supplying me with apparatus and medicine. I could go to his house every day if I chose. My wife was also ill, and Dr. Hunter attended her. The first month that my wife went he charged seven guineas for the two; but he afterwards reduced it to five guineas per month for the two, or three guineas for one. My health is now generally good. Now and then, when the weather is bad, I do not feel quite so well. I return to France at the end of this month.

Cross-examination.

Before I went to Dr. Hunter, I consulted Dr. Davis, of Finsbury-square. He is attached to the London Hospital. About two months before I went to Dr. Hunter, Dr. Davis examined me. I paid him only one visit. He said that he would give me something that would relieve me. He gave me a prescription. I saw Dr. Duke, of Norwood, about Christmas last, for quite a different complaint. I saw no one but Dr. Davis for this complaint before Dr. Hunter, and no one since. The expectoration was different from what I had been accustomed. Palpitation came first. My health had been generally

good until I had this complaint. I was 52 years of age last April. Dr. Hunter said the upper lobe of both lungs was slightly affected. I inhaled at my own house only, and always with the same apparatus. I went to Dr. Hunter once or twice a week. He examined me about once in every three weeks. I have been to him occasionally since the three months. He gave me something in the bottle to inhale, and an embrocation. I had lost flesh quickly before I went to him, and gained it quickly after I went to him. When I went to him I weighed 133 pounds, and after three months I weighed 151. My weight, when in good health, before had been 148lbs. Except telling me that the upper lobe of my lungs was affected, Dr. Hunter never told me what was the matter with me.

Bernard Janin Sage.—I am an advocate of the American Bar, and have been practising in New Orleans. I had under my charge here the son of a friend of mine. He became ill in 1865. I placed him under Dr. Hunter's care, where he remained for six or eight months. He is now at Edmonton, at school. I was present when the youth was stripped and examined by Dr. Hunter. There has not yet been any settlement of the charges of Dr. Hunter.

Bernard Janin Sage (recalled).—Having examined the New York statutes, I am enabled to say that the law of New York, with reference to the University of the city of New York, is this :—The University is incorporated by statute, and by the Act 11th September, 1837, the medical department of the University was established, and by that Act the medical faculty of the University were authorised to grant diplomas or degrees of Doctors of Medicine, and these diplomas or degrees were declared to be licences to practise medicine and surgery. No person is to receive such diploma unless he has pursued the study of medicine for three years after being sixteen years of age, and shall have attended two full courses of lectures in some medical college, the last course being in the University of the city of New York. I do not think that there is any provision as to examination. This statute was in force in 1861.

I have consulted the statutes of New York, at the British Museum, since I was here last, and speak chiefly from that, having read them as a lawyer.

John George Meyer. I am a commercial broker, and live in New York. I am now staying at 19, Camden-square, Camden-town. I know Dr. Hunter, and have been in the habit of consulting him. I first consulted him on the 21st of February, 1863. I was complaining of consumption; cough was incessant. I had hectic fever, night sweats, and expectorations of mucus and blood, as much as a pint and a-half in one day. I suffered also from general debility, emaciation, short breathing, and sores in the mouth. I could walk only with great difficulty. One day I dropped down in the street from exhaustion. I had been under the medical treatment of Dr. Macready, of Nine-street, New York, from the beginning of October,

1862, until a short time before I went to Dr. Hunter. When I consulted Dr. Hunter he was practising in New York. First of all he examined me, and then he gave me an inhaler, and a solution to put into it, with hot water. He told me to use the water as hot as I could without breaking the vessel, and to inhale three times a day before meals, once before each meal. I was to inhale each time for 15 or 20 minutes. I also went to Dr. Hunter's house every day for several months. I inhaled there each day, but only for a few minutes. I inhaled there camphor vapours, and oxygen as a separate operation; first the camphor vapours, and then the oxygen. I could not stand camphor for more than a few minutes—in my opinion no one could. There was a room specially fitted up for inhaling. There was one inhaler for the camphor vapours, and one for the oxygen. It was a sort of copper kettle, covered over, and with a small aperture, that is, a small pipe to guide the vapour. I stood over it, and inhaled. Dr. Hunter also gave me an embrocation to rub on my chest, to produce an eruption and counter-irritation. I rubbed it on each side of the chest alternately, so as not to have both sides sore at the same time. He told me also that I was to be very particular about my diet, and take good nutritious food; to live regularly and well, to keep my chest warm, and to be very careful. He ordered me to take a medicine, which he said was a compound or solution of iron, a teaspoonful in a wine-glass half-full of water after each inhalation. The relief, after the first two or three days, was immense. I never would have believed it. First of all the sores in the mouth disappeared; the hectic fever and the night sweats ceased after three or four days. I speak from recollection, my illness being impressed on my memory. The cough was less violent; I could sleep better at night, and the expectorations were less; my appetite improved. I felt stronger, and then began to get a little stouter. I continued the treatment, more or less, until I left New York in May last. I did not latterly inhale more than once a week. I came to feel tolerably well about twelve months after I first saw Dr. Hunter. I began to feel almost entirely well, except a little weakness in my lungs, which is all that now remains. I have increased 20 pounds in weight since I have been under Dr. Hunter's care. His charges were 25 dollars the first month, 25 dollars the second, 20 dollars the third, and 15 dollars per month afterwards. I have seen a great many other patients at Dr. Hunter's. I have consulted him since I arrived in England, and have been treated medically by him. He has given me solutions to use in the inhaler as in New York, and a tonic after each inhalation, and a cough mixture to be used occasionally, and pastilles to be burnt in my bedroom when I go to bed. I intend to return to New York in October.

I have no objection to be examined by Dr. Bennett on behalf of the deferdant; but I do object to be examined now.

I began to feel ill in July, 1862. I was then living in New York.

My illness began with cold chills in the summer. I had no medical advice until October, 1862, and then I went to Dr. Macready. He attended me until February, 1863, and from that time until last May I have been under Dr. Hunter's treatment. I left off regular inhalations about twelve months after I had seen Dr. Hunter, and after that I inhaled occasionally when I considered my symptoms required it. Dr. Hunter attended me personally two or three months, then his brother, Dr. James Hunter, and then Dr. Hunter's assistant. I began to feel really better and to get my strength back about six weeks after I first consulted Dr. Hunter. Dr. Macready treated me by giving me continual opiates for stopping the cough, and quinine and also cubebs to stop the irritation of the cough, and other things were given to me. I used to get better one day and worse the next. The only relapses after going to Dr. Hunter were when I did not take care of myself, but the whole tendency was for the better. Dr. Hunter did not tell me I had Consumption, but I heard it from a friend of mine. Dr. Hunter may have told my wife. I knew very well I had severe inflammation of the lungs, by the symptoms which I have described. I do not think I ever asked Dr. Hunter what was the matter with me. I suffered so much from cough, fever, debility and expectoration, that there was no necessity for me to ask. Dr. Hunter did not tell me what it was I suffered from. I asked no questions. I am much better now than I was. I went to see Dr. Hunter after my arrival here, because I did not feel well. I had pain in my chest, and occasionally a cough. I lived in New York for thirteen years. It is a very cold place in the winter. Dr. Hunter told me to go to his house whenever I liked. Dr. Hunter lived in Broadway, about three-quarters of a mile from me. I used to go to him generally between three and four. I attended to my business in the afternoon, as well as I could. I often went in a carriage, because I could not walk. Dr. Hunter told me it was oxygen I inhaled. That was through a bottle filled with water, and a pipe going into it. It was only at his house I took this, about ten whiffs at a time. That I did only for the first two months, and I went on with the camphor vapours at his house for two or three months. I did not see it prepared, but I was told by Dr. Hunter that it was oxygen. I do not know what the inhalations were at my own home. They were changed frequently, according to my condition. He supplied the medicines himself. No member of my family has been consumptive. When the weather was very rough I stayed at home two or three days at a time, but when fine I walked out and attended, as far as I could, to my business. Dr. Hunter examined me the first time I went there. I stripped to my waist, and he examined me with an instrument which, I believe, is called a stethoscope.

THE DEFENCE.

As it would be impossible for the reader to carry in his mind the whole evidence adduced by the defence, and apply it while perusing my refutation of its several points, I have divided it into separate portions, each one of these having some bearing upon a distinct part of my theory, descriptions of disease, or mode of treatment. I shall also make use of figures to denote the passage on which I comment.

I will premise, however, that I shall not attempt to do more than show, that on every point the views published by me have been misrepresented and most unfairly assailed. To attempt to prove that I am *right* in theory or opinion, and my opponents *wrong*, would be to undertake what is uncalled for by the object of this publication, and not necessary for my vindication. If I show that their evidence was, on most points, based upon a *palpable*, if not *intentional and pre-arranged, misinterpretation* of my words, and that every paragraph quoted from my descriptions of disease can be paralleled from the writings of eminent members of the profession, I shall accomplish the end I have in view.

DR. CHARLES JAMES WILLIAMS, *sworn*.

Examined by MR. QUAIN.

You are a Fellow of the Royal College of Surgeons, I believe?—I am.

And Consulting Physician to the Brompton Consumptive Hospital?—I am.

And I believe, in the course of your practice and study of the profession, you have paid considerable attention to the diseases of the lungs?—Yes.

And you have been in practice how many years in London?—I have been in practice in London since 1830—six and thirty years.

Now have you read the work of Dr. Hunter called “Dr. Hunter’s Letters?”—I have.

Now I am going to call your attention, in the first instance, to a few

passages of the work. If you will take a copy of it in your hand I will be obliged to you?—I have a copy of the work.

Now in page 87 is the first passage to which I would call your attention, and you will find there this statement:—"All observation "and experience unite to prove that the root of the malady," meaning consumption, "is in the lungs, and the tubercles are but the fruit of "imperfect respiration." Now, I will ask you, according to your experience, and according to the experience of the profession at large, is that a correct statement, or is it not?—I say distinctly that it is as nearly opposed to the truth as a statement can be.¹ It is a remarkable fact that many of the diseases in which the respiration is most imperfect are remarkably exempt from disposition to tubercle. Several kinds of disorder, in which the respiration is imperfect, have a remarkable freedom from the tubercular disposition. Of these I would mention particularly emphysema of the lungs, in which the air cells are dilated, and there is a perpetual difficulty of breathing, and yet it is extremely rare for that ever to terminate in tubercle; and, in fact, it is considered in the profession that extensive emphysema of the lungs is incompatible with tubercle. I may mention, for the understanding of the jury, that emphysema of the lungs is a professional term for habitual asthma. The same remark will apply also to spasmodic asthma.²

The Lord Chief Justice. I do not understand this passage to say that there cannot be imperfect respiration without tubercle?—It is stated that tubercle is the fruit of imperfect respiration. Imperfect respiration is the tree, and the fruit of that must be tubercle. You cannot have the fruit without the tree.

May not imperfect respiration sometimes produce tubercle? I do not understand him to say that it always does. Where you find tubercle, it is the fruit, you see, of imperfect respiration?—By itself it does not appear that imperfect respiration can produce tubercle. There is another class of cases which exemplify imperfect respiration, connected with diseases of the heart, in which the venous blood is circulated instead of the arterial blood through the system. These also are cases remarkably exempt from tubercular disease.³

Mr. Quain. Although accompanied by imperfect respiration, as I understand?—Although accompanied by imperfect respiration. Another class of cases may be mentioned—those connected with extensive distortion of the spine, owing to which the mechanism of respiration is impaired—and it is the general experience of the profession that tuberculous disease is not common in those cases.⁴

The Lord Chief Justice. Suppose imperfect respiration to arise from some derangement of the air passages?—That commonly arises in the course of bronchitis, and also spasmodic asthma.

Does that derangement, and the imperfect respiration arising from that, *tend to produce* tubercular disease?—By no means in itself. In itself it is not capable of producing it.⁵

¹ The furthest removal from my statement would be its exact opposite—viz., that imperfect respiration *prevents* tubercles. If this be true, to place those who are strongly predisposed to Consumption under such influences as tend to impair the function of the lungs, would be, on Dr. Williams's theory, a most salutary proceeding, and one very likely to prevent the development of Consumption!

I say, on the contrary, that *imperfect respiration* always precedes the development of tubercles; and hence, that whatever *impairs* the function of the lungs, directly tends to *produce* Consumption. Under the head of imperfect respiration, I have, in my book (p. 85), explained that I embrace in the term—firstly, the respiration of *impure* air; and, secondly, a diminution in the *quantity* inhaled.

The impurity may be *dust, smoke*, or any *foreign* matter in the air, or the *carbonic acid* exhaled from the lungs, which, in badly ventilated and overcrowded rooms, *accumulates*. The diminished quantity may result from *local* diseases, as Catarrh, Laryngitis, Bronchitis, &c.; which, by obstructing the air-tubes, diminish the current of air drawn in at each breath; or it may be caused by a contracted chest, or by any occupation which confines the motions of the chest and habitually prevents its proper expansion.

Having thus cleared the ground and shown what I fully explained in my book of letters, as being embraced in the causes producing "imperfect respiration," the elucidation of which causes occupies twenty pages of my book; let us now take up Dr. Williams's statement, and see whether it is as *near to the truth* as a statement made on oath ought to have been. I will take first, Dr. Charles J. B. Williams's (the witness) *Principles of Medicine* (p. 10).

"The *imperfect* performance of the functions of *respiration*, and cutaneous "transpiration in small and close habitations, make persons offensive both to themselves and to others, in consequence of *corrupt matter*, which ought to be "eliminated, being *left in the system*."

Again, p. 53—

"The habitual want of *pure air* especially exerts an unfavourable influence on

"the state of the blood, and on the functions of circulation and respiration, causing pallidity of the surface and imperfect development of the corpuscles and plasma, which then, instead of contributing to the nourishment of the textures, *degenerate into scrofulous or tuberculous matter, whose deposition in the internal organs or glands is further favoured by the weakness of the circulation.*"

This is precisely what I have said in my book (p. 80). Impure air, by preventing the decarbonisation of the blood, lays the foundation for tubercles. The following are my words:—

"Every room occupied by human beings, or even by brutes whose lives are valuable, should actually be made to *breathe*—that is to say, should throw out a steady stream of *foul air* and take in a steady stream of *pure air*. When this is not attended to, the occupants soon become feeble, their cheeks lose their freshness, their blood becomes thick, dark, and carbonaceous, and, ere long, their disregard of the laws of health is punished by premature *decline*."

I am really unable to distinguish between Dr. Williams's *written* opinions and my own on this point. When he wrote the passages quoted above he was probably thinking of the *disease*, but when he gave his *evidence* he was probably thinking of *me*!

ANCELL says—

"The most efficient of the *true* causes, or those influences which are *most* powerful in inducing the disease [consumption] is a vitiated atmosphere, insufficient muscular exercise, and the depressing passions. The whole of these influences meet, as it were, at one common point, and have probably one essential *modus operandi*, viz., an insufficient renewal of pure atmospheric air, or an altered condition of the air habitually contained within the chest" (p. 545).

"M. Banelocque regards the respiration of an atmosphere not sufficiently renewed [stagnant, as in close rooms] as the *sole* cause of tuberculosis. Giving this expression its true interpretation, it means that the air within the chest, by which the true process of respiration is effected, contains habitually a larger proportion of *carbonic acid* and nitrogen gases, and a *smaller proportion of oxygen* gas, than is consistent with the due reaction of the blood." (Ansell, p. 444.)

So that impaired respiration is here laid down, even by Dr. Williams, as one of the causes of that very condition of the blood in which tubercle takes its rise. He *assumes* that some undefined change does take place; but what it is, he does not pretend to know. A patient goes to him with tubercles in his lungs; and, on investigating the case, he finds that he can trace it back to some "severe cold," or neglected bronchitis, pleurisy, or pneumonia; but no farther. He may assume that there pos-

sibly existed something antecedent; but he can neither explain what it is, nor does he pretend to say that the tubercles have not been produced by imperfect respiration. On the contrary, he admits that this cause alone will induce tubercles. Of what value then is this assumed something to either the physician or the patient? Practically he admits that consumption only comes under the eye of the physician *after* the disease has established itself in the lungs, and fastened its roots there. All beyond that is speculation and idle theory. It will neither prevent the disease, nor help us to cure it. There may be gold mines in the mountains of the moon, but who can prove to us that there are? We know just as much about them, as Dr. Williams does about this "assumed" state of the blood, on which it is *supposed* tubercles depend. Hear what he says—

"So, also, if we attend to different cases of pulmonary consumption, we shall find some in which the individuals had enjoyed very good health until they were attacked with one or more severe colds, or inflammations of the chest, or a fever, accompanied by pectoral symptoms, sooner or later after which the consumptive disease commenced" (p. 172).

² Three diseased conditions are mentioned. 1st. Asthma. This generally occurs in persons having large and even well-formed chests; and is, in the majority of cases, little more than *spasmodic Bronchitis*. The Bronchitis does not induce tubercles, because in the interval of the attacks the blood is generally properly decarbonised. The large size of the lungs enables the patient to inhale sufficient air to keep the blood pure. A condition of the air tubes, which ends in Asthma when the lungs are large, is very prone to end in Consumption when they are small. Then, again, as a rule, the imperfect respiration of Asthmatic cases is only *occasional* and *temporary*, and is followed by a much longer period of *perfect* respiration, during which the carbonaceous impurity of the blood, acquired during the fit, is effectually removed. The respiration is sufficient for all purposes during quiet. When the circulation of the blood is quickened by exercise, there is a demand for more air, and hence *shortness of breath*, but this also exists in a less degree in persons in health, and accounts for the panting respiration which we

experience after running or ascending a hill. Lastly, Asthma does very frequently run into Consumption.

What is true of Asthma is also true of heart disease. The paroxysmal character is the rule, and continuous dyspnœa [difficult breathing] the exception. Besides, in these cases, we often find that the oppression in the lungs is wholly due to distension of the capillary vessels, owing to the impeded flow of the blood to the left side of the heart, as in mitral and aortic valvular disease. So also in enlargement of the right ventricle, the hurried breathing is due to the rapid circulation. The *air* must meet the *blood* in the lungs; and if the current of the latter be *rapid*, the breathing will be *quick*. But this is not what is meant by *imperfect* respiration; since, the lungs being free, more than enough oxygen is brought into relation with the blood. It is an evidence of dyspnœa, but it is not an evidence that the supply of air to the lungs is insufficient.

³ The idea that those suffering from morbus ceruleus are less liable to Consumption than others, is one of those crude conceits which observation has exploded. Dr. Williams may still cling to that conceit, but, if so, he knew that his colleague, Dr. Walshe, repudiated it, and justified his repudiation by indisputable facts. But this is not all; Dr. Williams gives, in his own book, the result of Dr. Chambers's investigation of 2,500 *post-mortem* examinations, recorded in the books of St. George's Hospital, which was that the occurrence of tubercles was in proportion to the *venosity* of the blood. Now, knowing these facts, was it the act of an honourable or conscientious man to make a statement upon oath before a non-medical court, which was certain to be adopted by the Judge and the Jury as a medical *law*, or at least an indisputable *fact*, when he knew that at best it was a *mere opinion*, and that too a controverted opinion?

⁴ Distortion of the spine arises from various causes. It is frequently due to deposit of tubercle in the bone; and then the patient almost certainly has tubercle in the lungs, and dies of Consumption, and generally within a short period of time. Imperfect respiration is a common cause of tuberculous deposit in bone from which this deformity arises, and that it should exist is

natural enough. In spinal deformity, arising from insufficient or excessive power in certain muscles of the back, there is seldom any imperfect respiration; the chest becomes altered in form, but far less in capacity than is generally supposed.

⁵ But what would it prove if all that is *assumed* by Dr. Williams were true? Why, merely that we may have defective respiration without tubercles—that imperfect respiration does not always end in Consumption. Does *every* person exposed to the infection of fever take fever, or to the infection of small-pox take small-pox? If they did, the world would soon be depopulated. But is it not true that you cannot take small-pox without you *are* exposed to it? So also is it equally true, in my judgment, that a healthy man or woman cannot get Consumption without the function of the lungs first becoming impaired, through some one of the various causes which tend to destroy the balance between the oxygen of the air and the carbon of the blood.

Mr. Quain.—I will take you to another passage of the book, if you please. At page 90, in which these words occur:—"Tubercles are the consequence of imperfect oxygenation of the blood: their base is carbon." And then these words occur—"Carbon being a poison most inimical to life, I regard it as the true cause of tubercle. What does chemistry tell us of the essential nature of tubercle? The analysis of Scherer proves that 54 per cent. of it is pure carbon, and the remaining 46 per cent. made up of the elements of disintegrated tissues of the lungs in which it is deposited." I will ask you your opinion of that—"Carbon being a poison most inimical to life, I regard it as the true cause of tubercle."

The Lord Chief Justice. Divide the question.

Mr. Quain. First, with regard to the first statement of "carbon being a poison inimical to life, I regard it as the cause of tubercle?"—The first part, with regard to carbon being a poison inimical to life, is, in my opinion, an erroneous view as contrary to the truth as can well be stated. Carbon is the same as *charcoal*, and charcoal, whether in substance or powder, is a remarkable inert substance in the animal economy. It is occasionally administered as a medicine on account of its antiseptic properties.¹

(1) In speaking of carbon in the human body, I spoke of it as one of the elements of the organism, and not as an inorganic body. Pure inorganic carbon is found only in the *diamond*. Now Dr. Williams might with as much reason

have assumed that I meant diamonds as "*charcoal*." He knew perfectly well what it was I did mean, but he relied upon the probable ignorance of the Court, and presuming upon that ignorance, he could not withstand the temptation of dealing me what he considered a heavy blow, insidiously and unfairly. Carbon exists in the blood chiefly as a *hydro-carbon*, and it is only decomposed by the combination of oxygen, first with the hydrogen, and secondly with the liberated carbon. Every word he spoke, therefore, on this point was simply an evasion, and an imposture on the Court. In a popular treatise, it would have been an absurdity to make a distinction between carbon and hydro-carbon—between carbon uncombined and carbon in chemical union with hydrogen. But as I was speaking of carbon in the living body, no medical man could have mistaken my meaning except wilfully.

The Lord Chief Justice. He is not speaking of it there as a medical agent?—But I was going to show how it differs. Large doses, almost any dose of it may be administered without doing any harm.

The Lord Chief Justice.—You do not mean to say that it must not be got out of the system, when its usefulness there has been spent and gone?—Certainly.

But that is what he means?—It is represented here as an active poison most inimical to life.

But he does not say if you take it into your stomach it will destroy you; it is, if it is left as a portion of disintegrated tissue not got rid of. You must look what his meaning is?—I will explain. Even in the lungs it is innocuous. It is perfectly well known that there is carbonaceous matter in the lungs; and the lungs of aged persons show considerable deposits of carbonaceous matter which have lain there for years and years, which produce no other effect than of occupying parts which ought to be otherwise occupied. It is merely a mechanical agent, and not active as a chemical agent.²

Mr. Quain. Take the rest of the statement now—"I regard it as the immediate cause of tubercle?"—Well, that statement to me is unintelligible. It is totally inconsistent with medical experience.

² Even in the lungs, says Dr. Williams, it is innocuous. This was in reply to the Chief Justice, who required him to take time to consider what my meaning was. He was under oath, and his answer should have been such as he would have given if his Lordship and the jury had known the distinction between carbon and hydro-carbon, and he had been asked if hydro-

carbon in the tissue of the lung was innocuous. Now what are the facts? Why Dr. Williams in his own work speaks of this very deposition as occurring "in connection with a feeble circulation and with low respiratory powers; and it is promoted by conditions which increase the hydro-carbon in the blood." (*Williams's Principles of Medicine*, p. 451.) Again he says, "It has been surmised that this oil is the principal material out of which the bile is formed by the secreting cells; *that its great increase in consumption is mainly due to the accumulation in the blood of hydro-carbon*" (p. 448); so that hydro-carbon *does* in his opinion accumulate in the blood as the result of imperfect respiration, and he also knew that this had been supposed by others, besides me, to be the essential element of tubercle.

Now take the next statement if you please—"The analysis of Scherer proves that 54 per cent. of it is pure carbon, and the remaining 46 per cent. is made up of disintegrated tissue of the lungs in which it is deposited." Are you acquainted with the analysis to which reference is made here?—Yes, I am; I have an extract from the book that will enable me to speak. This statement of the analysis of Scherer is an incorrect statement; it states that 54 per cent. is carbon, which is true; it does not amount to as much as 54 per cent. quite, but it is 53 and a fraction, and "the remaining 46 per cent. is made up of disintegrated tissue in which it is deposited."

The Lord Chief Justice. Fifty-three and a fraction of carbon you say?—Fifty-three and a fraction per cent.: so far it is right.

Now as to the other part?—The remaining 46 per cent., according to Scherer's analysis, instead of being made up of the elements of disintegrated tissue of the lungs, should be represented by the oxygen, hydrogen, and nitrogen of the ultimate analysis; therefore the statement is incorrect.

When tissue is disintegrated, what does it consist of?—It consists of four elements: carbon, oxygen, hydrogen, and nitrogen.

Then, if you have so much carbon, the residue must be of the other elements, must it not?—Yes; but then that is the ultimate analysis in Scherer's analysis of the tubercle. It is the tubercle which consists of these four elements, of which carbon represents 53 and a fraction per cent.; the remaining elements are not elements of disintegrated tissue taken separately, but the elements hydrogen, oxygen, and nitrogen; and it is clear that Dr. Hunter has made a mistake in giving that analysis.

Mr. Quain. Let me ask you, then, does tubercle, as shown by analysis, contain more carbon than any other of the tissues of the

body?—On the contrary, it contains less than the ordinary principles of the blood.¹

¹ Here we have Scherer's and Glover's analyses of four distinct specimens of tubercular matter:—

Scherer	. . .	53·888	of carbon in 100 parts
Glover, 1st	. .	53·43	” ”
Glover, 2nd	. .	54·43	” ”
Glover, 3rd	. .	56·40	” ”

So that from 53½ to 56 per cent. may be assumed to be about the range.

Then Dr. Williams says it contains *less carbon than the blood*. Muller gives us the following elementary analysis of arterial blood—carbon, 51·382: showing an excess in tubercle of 2·506 of carbon by the analysis of Scherer, and an excess of 5·012 by the last experiment of Dr. Glover.

Mr. Quain. Then, as I understand, 54 per cent. is no peculiarity of tubercle; but the general animal tissues of the body contain something about the same, or less?—Something about the same, or more. May I quote from Scherer the passage on the point?

Yes.—It is this, “Hence tubercle may be regarded as,” &c., &c. [read the analysis of Scherer], or less than the other constituents of the blood; less than fibrin and albumen.

Contains less than albumen?—Yes, contains less than albumen, of which the tubercle is supposed to be formed.²

² He says it contains less carbon than albumen. Here is the analysis of albumen by Gay-Lussac and Thenard, quoted by Muller:—

Carbon 52·883

The Lord Chief Justice. Tubercle is supposed to be formed from the animal ingredients?—Yes, from the organic ingredients of the blood; albumen and fibrine.

Not albumen alone?—It is difficult to say; albumen is continually undergoing change into fibrine. The general impression is that the materials are formed *from the fibrine*; but the fact is, that fibrine and albumen come so closely to each other that the distinction is not material.

Dr. Williams supposes that *the general impression* is that tubercle is formed “*from the fibrine*” because that is *his* view. Professor Hughes Bennett, on the other hand, distinctly says it is formed of “*albumen*,” and that is Dr. Hughes Bennett's view.

I will venture to say that of physicians in practice fully *nine* incline to the opinion of the latter to every one that does to that of the former.

Professor HUGHES BENNETT says—

“ The lungs become especially liable to local congestions leading to exudation of an *albuminous* kind, which is *tubercle*” (p. 35).

But even this is only *theory*.

Professor SWETT says—

“The precise nature of tubercle has not yet been ascertained. This doctrine [of Dr. Hughes Bennett] “is at best but an ingenious hypothesis” (p. 218).

Again—

“Those who maintain the opinion that imperfect digestion and the mal-assimilation of the food is a leading cause of the tuberculous deposit, have failed, I think, to establish the fact” (p. 254).

I, too, think they have failed “*to establish the fact*,” and claim the *right to say* so, and to advance my *theory* instead of theirs.

Mr. Quain. Now turn to page 78 of the book; and, after what you have told us, is this statement correct at the end of that page, after stating that consumption is only this carbonaceous impurity of the blood, “Tubercles themselves are only deposits of carbon in the tissue of the lungs, and the ulceration and destruction of this important organ result directly from their presence.” You see, in the early part of that passage it is stated that tubercles are only deposits of carbon in the blood?—I think, from what I have already stated, that it may be inferred that that is totally incorrect, and contrary to the fact.

This, again, is putting a false construction on the word “*carbon*.” He means that tubercles are not “*charcoal*.” As I never said they were, but that carbon was the *base* of tubercle, and that tubercle contained about 54 per cent. of carbon, which was an excess over “*animal tissues*,” his evidence on this point was a mere imposture upon the Court.

Now direct your attention to page 10 of the book, where there is this passage, beginning at the first paragraph, “Catarrh is the first step toward consumption. When allowed to go on, it soon involves the throat in granulations,” and so leading on. Tell me whether, in your opinion, and in the opinion of the profession, that is correct?—I think I may take it as a gross exaggeration of the fact. It cannot be denied that catarrhs and colds may, in persons predisposed

to consumption, develop the disease—be the first stage in the development of the disease; but to state, as it is stated here, that catarrhs, and sore-throats, and bronchial affections generally lead to consumption is a gross exaggeration of fact.¹

The Lord Chief Justice. I think it is stated with this qualification, that he is speaking of them as *chronic diseases*, when they have become chronic?—I think it says merely “neglected.”

But you see, he works it out from simple catarrh up to chronic catarrh; and it is when he has gone through the statement about catarrh that he says it is the first step of consumption.

Mr. Coleridge. And you will see, lower down, he says they are mild and easily cured in the commencement, but become grave and dangerous when neglected.

The Lord Chief Justice. That is so. Be careful not to make him say more than he does say. If you take a single passage from any scientific treatise, you are very likely to misunderstand what the author means. It is necessary to look upon it as a whole. And you will see, if you read it carefully, it is of chronic catarrhs that he is speaking when he says that catarrh is the first step towards consumption?—Well, take *chronic catarrhs*, or *neglected colds*, it is very rare you find that such affections terminate in consumption where there is no predisposition to disease.

(1.) As he has not given us any description in his works, of Catarrh, he probably knows very little about it practically. Chronic Catarrh is one of the manifestations of that very *scrofulous* condition of the blood for which he contends, and hence is an outward sign of the tuberculous predisposition. Catarrh, as described by me, is a disease attended by thickening and ulceration of the membrane of the nose, by the formation of false membranes within the nasal passages, and by purulent and offensive discharges from the posterior nares. Now, if this condition be of a scrofulous nature generally, and if Scrofula and Tuberculosis be identically the same disease; then, if the health of the patient is to be saved, we cannot surely begin too soon. But he denies all this, and so compels me to prove it.

Professor GROSS states that—

“Ulcers of the nose, chiefly of a *scrofulous* (strumous) nature, are sufficiently common, and from their rebellious character and fetid discharges, are often a great source of annoyance, both to the patient and the practitioner (p. 414). In the more aggravated forms of the affliction, large quantities of inspissated mucus pass off; or, collecting in the nasal cavities, form thick brownish incrustations, which drop off every fourth, fifth, or sixth day.” (p. 415.)

ANCELL says—

“ In tuberculous subjects, the membrane lining the nose is extremely liable to copious and acrid secretions. It is also extremely liable to chronic inflammation, and muco-purulent discharges. . . . Many cases of ozæna and diseases of the antrum and bones take their origin in *tubercular* deposits.”

Professor HUGHES BENNETT tells us that—

“ The disposition to pulmonary Consumption is very much increased by circumstances, which keep up constant cough.” (P. 220.)

Again—

“ Here, as in several other instances, I have seen pulmonary symptoms, and especially cough, were altogether dependent on the nasal disease.”

Dr. Williams admits, in his own book, that as the disease comes under the physician's care, we find “some in which the individuals had enjoyed very good health until they were attacked with one or more ‘*severe colds*.’”

“ In others again, the cough and other symptoms begin very gradually, without any obvious cause; and with as little apparent external reason, soon increase to a serious extent, and the Consumption runs a rapid career. In a third class of cases, the patients have been out of health, in a debilitated cachectic state, before the commencement of the cough, and other local symptoms, which become suddenly developed after exposure to cold.” (P. 172.)

Again he tells us—

“ In the first class of cases, we have the development of Consumption from local inflammation. . . . The acute inflammation, whether pulmonic, pleuritic, or bronchial, *imperfectly* treated and only partially subdued, passes into a *chronic* form, and either immediately develops *tubercular* indurations in the lungs, or by lowering the vital powers generally, leads to their formation.” (Page 172.)

Again—

“ Under any of these circumstances, chronic inflammation, either by its own local effects, or by its depressing influence on the constitution, or on both combined, becomes a sufficient cause of Consumption.” (Page 173.)

Now this last is precisely what I have myself said in treating of “Catarrh,” “Laryngitis,” and “Bronchitis,” in the *chronic* form. Do they not impair respiration? Why, difficulty of breathing through the obstructed air-passages is the characteristic symptom. Again, Dr. WILLIAMS, in his *book*, says—

“ Among the *local causes of Consumption* are to be reckoned also the habitual inhalation of fine solid particles, which is contingent on certain occupations, such as those of needle-pointers, &c.”

Now, as all these local diseases and influences are embraced in the list given by me in my book, under the head of "Causes of Consumption;"—as "imperfect respiration" is the inevitable consequence of such affections; and, moreover, as Dr. Williams must have known that many high authorities regarded imperfect respiration as the sole cause of Scrofula and Consumption, how can he reconcile his evidence with a proper regard for veracity? The impression he created upon the minds of the Jury was, that imperfect respiration did not exist, and could not produce tubercles.

That evidence is disproved by Professor HUGHES BENNETT, who says—

"Lesions in the throat, larynx, bronchi, and nasal passages, ought to "occupy the serious attention of the practitioner in all cases of pulmonary "disease." (Page 221.)

And he adds—

"That to treat these local affections, not only affords the patient relief, "but tends in a marked manner to induce *arrestment* of the pulmonary disease."

Now, if *chronic* Catarrh be generally an evidence of that condition of the health which ends in Consumption; if tubercles are often deposited in the nasal passages; if it does tend to keep up cough; and if treating it will contribute to arrest the progress of disease in the lungs, I think the sooner those afflicted with Catarrh are treated, the better for themselves, at least. If Ancell, Hughes Bennett, and Gross, are not all *gross-deceivers*, all these things are correct, and Dr. Williams *again in error*.

Mr. Quam. Then look at page 33, in which this passage occurs: "In my last letter I concluded my observations on the diseases of the "nose, throat, and bronchial tubes, and I now come to speak of "pulmonary consumption, that dread malady in which these affections, "when neglected, invariably end." What do you say to that?—That is another instance of this gross exaggeration and mis-statement, the tendency of which seems to be merely to excite alarm.

The statement is repeated in rather stronger language, in page 38: "In former letters I pointed out the danger of neglecting 'catarrh,' "sore-throat,' and 'bronchitis,' and told you that these are the common "causes of consumption; it would have been more correct to have "said that they are the cause of tubercles, and end in consumption."

The Lord Chief Justice. It is the same thing in effect.

The Witness. Yes.

Mr. Quain. Now the next passage to which I shall call your attention is in page 44. At the end of the page there is this passage: "In health the pulse should range from 60 to 68, the average being 64 beats in the minute. If, therefore, you have a hacking cough, and slight shortness of breath on exertion, accompanied by an increase in the frequency of the pulse of ten or fifteen beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal." Now is that a correct statement in reference to the pulse, and of the consequences of an increase of from ten to fifteen beats per minute?

The Lord Chief Justice. Divide the question.

Mr. Quain. In health the pulse should range from 60 to 68, the average being about 64 beats in the minute. Is that correct?—That is rather a low standard, I should say. Applying to the male sex it may be correct; but in the female sex the standard ranges higher than 64 or 68.

The Lord Chief Justice. Would that be 70?—70 beats per minute, or rather above it.

Mr. Quain. Now the statement goes on to say, having put down the average at 64, "If, therefore, you have a hacking cough, and slight shortness of breath on exertion, accompanied by an increase in the frequency of the pulse of ten or fifteen beats per minute, you cannot doubt the existence of mischief in the lungs." What do you think of that?—If that were true, I believe there are very few in this room that would be out of danger; for in every kind of catarrhal inflammation there is usually some acceleration of the pulse, some quickening of the breath, and in females, such an acceleration as is here mentioned—ten or fifteen beats per minute—is still more common, particularly in connection with a hacking cough.

The Lord Chief Justice. First, I understand you to say that in colds there is a liability to cough, and an increase of the pulse?—Yes, and a quickening of the pulse.

What do you say about females?—In females the quickening is more readily produced.

Not quickening by a cold?—Yes, the quickening of the pulse, not only in colds, my lord; many persons are subject to a nervous cough, and many such persons are subject to this acceleration of the pulse.

Mr. Quain. The statement in this case is, that you cannot doubt the existence of mischief in the lungs. Is that true?—I can only call that statement a very absurd statement.

The Lord Chief Justice. But do you understand him to apply that statement to any other than these symptoms as habitual?—I think, my lord, it does not refer to anything habitual.

You must read the whole chapter, and see what leads up to this. If you will read the whole chapter as leading up to this statement, does not it appear that he is speaking of these symptoms, as not arising from

colds or any accidental circumstances, but as being habitual; that makes a very material difference?—I do not so interpret it; it may be so.

You are a much better judge than I am whether it is so or not. I call your attention to it as affording a very large distinction.—Admitting that, I would still say that these symptoms alone do not.

But which is he speaking of? Is he speaking of the cause of cold, where those symptoms are produced temporarily and accidentally only, or is he speaking of them when they become habitual?—I do not think, as it is expressed here, that it is necessary that it should be habitual. The passage is, “If the pulse is increased ten or fifteen beats per minute.”

Are you in the habit of interpreting a man's book by a single passage from it?—No; but taking the whole chapter, I say that is the whole thing. There is a great deal obscure in the meaning of many passages.

Now, on this point, regarding the pulse, as on many others, much difference of opinion exists among medical men. Dr. Williams thinks from 60 to 68 correct for the male sex, but not quite high enough for the females. After all, is not this splitting hairs about nothing? Hear what Sir JAMES CLARK says. Speaking of the state of the pulse *in this disease*, he says—

“Eighty pulsations in a minute, which may be the natural number in one patient, is a frequent pulse in another, whose natural pulse is 60 or 65. In my opinion, the average natural frequency of the pulse is generally estimated too highly by authors.” (P. 98.)

Again—

“A frequent pulse, even taken as an isolated symptom, should *excite suspicion*, and when accompanied with other symptoms indicative of the tubercular disease, it strongly favours the presumption that mischief has already commenced.” (P. 99.)

Does not this show, conclusively, the disputable nature of the purpose to which Dr. Williams lent himself? He must have known that Sir James Clark had written even more strongly than I had done on this very point. The works of Laennec, Louis, Sir James Clark, and Sir Thomas Watson have been my text-books for twenty years. I naturally imbibed their precepts, and have adhered to them, except where experience has led me to modify them; this certainly is not one of the points. I believe Sir James to be strictly correct. But what do I say? Why, that the pulse must be from 80 to 85 or 88 *habitually*.

With this, there must be a *dry, hacking cough* and *shortness of breath*. If these three symptoms are found together and continuous, we cannot doubt that the free action of the lung is impaired. What the nature of the disease may be, I do not pretend to say; but whatever it may be, none but a fool would wish to shut his eyes to the danger, and wait until it had developed itself further; or would permit such a combination of suspicious symptoms to remain without carefully inquiring into their meaning. Dr. Williams deplotes, in *his* books, the fact that medical men never see the patient until the disease is far advanced, whereas it might generally be *cured* if only seen earlier. And yet, because I point out the earlier symptoms, and try to arouse sufficient interest in the minds of those suffering to induce them to go to the physician at the earliest symptom of disease, he comes forward and tells the public that these symptoms are of no consequence, or what practically amounts to that. If his words are believed, the lives of thousands who might be saved by early treatment will be sacrificed. What is the meaning of this monstrous folly? Does he mean to say to those who have a frequent pulse, a hacking cough, and shortness of breath: These symptoms are of no consequence; never mind them; wait and see what becomes of them. If that is his meaning, then all I can say is, that many will learn when it is too late that the advice has cost them their lives. No injury could result from inquiry. If the cause is slight, it will be all the easier to get rid of; if serious, it cannot be detected too early for the good of the patient. Putting the most charitable construction upon the matter, I view Dr. Williams's evidence on this point as a wrong done to the profession, and an outrage upon the afflicted.

Mr. Quain. For instance, taking the second passage in the chapter, at page 40, "I now come to speak of the symptoms which indicate " that the lungs have become 'affected,' that is to say, that tubercles " are deposited. It is no unusual thing to hear designated as 'a mere " cold,' or 'a slight bronchial affection,' symptoms which should excite " the greatest anxiety, as indicating the commencement of tubercular " deposition." That is, as I understand it, not a habitual cold, or a habitual bronchial affection.

The Lord Chief Justice. But he passes on and comes to this part: "One of the earliest signs of consumption is cough. For a considerable time this is so slight as to be entirely overlooked by the patient, and may scarcely be noticed by his nearest relatives, it being little more than an occasional dry hack. It is most commonly observed in the morning, on first getting out of bed, but it may also occur during the day, after meals, and after walking or conversing." And then he goes on to point out the different sorts. "Now, when a dry, hacking cough steals upon a person in apparent health, and without the occurrence of a cold, it should always excite apprehension, and lead to an immediate examination of the lungs. It may possibly prove unimportant, for dry cough does not always end in consumption; but it is suspicious, and no man who values health will disregard its warning." Then he goes on to speak about expectoration, and then as to shortness of breath, and then he goes through the different ways it comes to the form it assumes; and then, having gone through all that and the frequency of the pulse, he winds up with, "If, therefore, you have a hacking cough, and slight shortness of breath on exertion, accompanied by an increase in the frequency of the pulse of ten or fifteen beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal." That may be right or wrong, but it is certainly unjust to take a particular passage, and not read it by the light of the passage that goes before.

It is as well to *nail* these passages, out of which the witness is endeavouring to torture something to my prejudice, as they arise. As he does not like my description, perhaps the following is equally objectionable.

Sir THOMAS WATSON says—

"Cough is one of the earliest symptoms of consumption; and it is that which commonly first attracts the attention, and awakens the fears of the patient or the patient's friends. Generally at first it is slight, occasional, and dry: it occurs upon the patient getting out of bed in the morning, or if he makes any unusual exertion in the middle of the day. It feels to him as if it was caused by irritation about the throat. Sometimes it will cease for a time, as in the warm weather in summer, and recur in winter, when the external temperature is lower. By degrees it begins to get troublesome in the night, and to be attended with more or less mucous expectoration.

"Now when such a cough steals upon a person gradually, and when no reason can be assigned for its occurrence, that circumstance alone is enough to excite suspicion as to its true nature and cause."

Mr. Quain. You have read the whole book, I understand?—Yes. And are not speaking of isolated passages?—No.

The Lord Chief Justice. That is a leading way of putting the ques

tion, and certainly does not agree with what he said before. He said he judged of it by that passage.

Mr. Quain. With great deference, he said he had read the chapter.

The Witness. I think the commencing passage speaks of it as being no unusual thing to hear designated as a mere cold or slight bronchial affection, symptoms which should excite the greatest anxiety.

That is the commencement?—Yes. The commencement does not speak of neglect or duration.

Now, look, if you please, at the passage in the third line in page 47, beginning, "In young women particularly, I have very often found the lungs seriously affected, while they still retained their colour and plumpness. But as a rule, if we reduce the matter to a certainty, by weighing, we shall find a few pounds of difference between their present and former weight. If with the loss of weight there is a disposition to sigh, a dark discoloration below the eyes, and a quickened pulse, with some heat in the hands, set it down as almost certain that the lungs are affected." What do you say to that passage?—I would say that it is not founded in fact, so far as my experience goes; for these symptoms that are here indicated are so common in females that I should think almost fully a third——

The Lord Chief Justice. Just take the passages one after another. Is it a fact that in young women the lungs are sometimes seriously affected, but that they still retain their colour and plumpness; is that true?—I do not believe that is true; not affected with tubercle, observe, my lord.

Mr. Coleridge. That is not the question.

Mr. Quain. Seriously affected?—Still not with tubercle.

This is a distinct issue of fact, and it will be easy to show whether Dr. Williams's statement is correct or not.

Sir JAMES CLARK says—

... "in others the disease makes considerable progress before the patient becomes visibly thinner, examples of which I have found most common in *young females*" (p. 104).

Dr. COTTON, his own colleague, says—

... "sometimes the decrease is so rapid that it will attract the attention of friends; at other times it requires the *periodical use of the weighing-machine to detect it*" (p. 100).

Dr. WALSH, another one of the physicians to the Consumption Hospital, says—

... "the body may be *plumply fat*, while large cavities exist at the apices, and the disease makes rapid progress downwards" (p. 461).

Dr. Williams must have known that these opinions had been expressed by his colleagues. His *oath* was therefore directly in

the face of them. Sir James Clark mentions it as most common in young females, just as I do. Dr. Cotton says it often requires weighing to detect the loss of weight, it is so imperceptible, while Dr. Walshe uses the very word "*plumpness*" used by me.

The Lord Chief Justice. It will be much better to read the whole passage, beginning the other side. "The losing a little flesh is often an early symptom of consumption. It takes place in many cases even though the appetite remains good and the food is abundant and nutritious. When a patient grows thinner without any appreciable reason, the lungs should always be suspected as the cause. The same is true of those who rapidly gain flesh, and then as suddenly lose it. These changes merely indicate the changes taking place in the lungs. Still we do not always find apparent loss of flesh in the first stage of tubercles. In young women particularly, I have very often found the lungs seriously affected, while they still retained their colour and plumpness." That is the whole passage, and Dr. Williams can give us his experience whether it is so or not.

Mr. Quain. Take the whole passage read by my lord, and give us your experience about that, and your opinion of the whole passage?—The earlier parts of the passage may be admitted, "The losing a little flesh is often an early symptom of consumption." No doubt about that. "It takes place in many cases, even though the appetite remains good and the food is abundant and nutritious." That is also correct. "When a patient grows thinner without any appreciable reason, the lungs should always be suspected as the cause." That is strong language, overstating the case, but still not devoid of truth. "The same is true of those who rapidly gain flesh, and then as suddenly lose it." I do not consider that is applicable to tubercular disease. "These changes merely indicate the changes taking place in the lungs. Still we do not always find apparent loss of flesh in the first stage of tubercles." Well, I believe it is a very rare thing that it does *not* take place—loss of flesh. The subsequent statement is a very great exaggeration.

The Lord Chief Justice. About the young women retaining their colour and plumpness?—No, after that.

What do you say about that passage?—Retaining their plumpness? Well, that is a very rare case, if it ever happens. It is the latter part that I refer to.

Mr. Quain. Point out the part which you say is a gross exaggeration?—"If, with the loss of weight, there is a disposition to sigh, a dark discoloration below the eyes, and a quickened pulse, with some heat in the hands, set it down as almost certain that the lungs are affected." That, I say, is a gross exaggeration of symptoms often occurring in females totally irrespective of any disease of the lungs.

Hear what Sir THOMAS WATSON says on this point:—

"If, without any apparent cause, a person grows thin and weak, and his pulse is quick, and his breath at all short, these are intimations, which seldom prove unfaithful, that tubercular disease is at work in the lungs and in the abdomen' (p. 191, vol. ii.)

The Lord Chief Justice. Though they often occur where there is no disease of the lungs, do they occur where there is disease of the lungs?—Yes.

Mr. Quain. Now I will call your attention to a passage in page 33, "Consumption is the most fatal disease in the long catalogue of bodily afflictions, and hence appeals more strongly than any other to the physician for help. In this, and indeed in all countries lying within the temperate zone, it causes one death in every four which result from disease. The deep interest and great importance of everything which promises to mitigate the ravages of this scourge, cannot, therefore, be over-estimated. The weekly bills of mortality point us to the startling fact, that of those who have passed the age of puberty fully one in every four persons we meet in the great thoroughfares of business and pleasure is under the ban of this terrible disease, and destined to fall a sacrifice to it, unless saved through the prompt adoption of more rational means than those usually employed. No man, however strong of constitution, is proof against its silent and treacherous approaches." Now I ask about the beginning of the passage, is it a correct statement that, at the time this book was published, in all countries lying within the temperate zone, one death in every four was the result of consumption?

Mr. Serjeant Parry. "Which result from disease," the book says?—I believe statistical inquiries have put the average number considerably lower than that. The average bills of mortality, or returns of the Registrar General, for the year 1865—1864 or 1865, up to that period, was about 1 in $8\frac{1}{2}$ in the whole.

The Lord Chief Justice. That is including all deaths—not from disease alone?—Deaths from disease.

With reference to the 1 in $8\frac{1}{2}$, does not that $8\frac{1}{2}$ include deaths arising from other causes than disease?—Yes, all cases of death. I speak from memory. That is a point on which I am not sure.

If you have not ascertained yourself, we will ask another gentleman who has?—I speak entirely from memory, not having consulted the figures of late.

We will go on to another matter then. At page 112 the paragraph begins:—"From these facts the reader can understand that one of the great objects of treatment is to subdue this catarrhal condition of the mucous membrane. How can this be accomplished? It is no treatment for a local disease in the lungs to pour cod-liver oil and tonics into the stomach, for they never reach the part affected; and, besides, such medicines possess no properties capable of effecting cure if they did. No physician will pretend that cod-liver oil, or any cough mixture or tonic ever compounded, has power to remove tubercles,

purify the blood, or heal the mucous membrane, even if directly applied. How then, in the name of reason, can they accomplish these objects, when they are applied to a distant and a healthy part? They are merely palliatives—nothing more—and nobody, unless it be the patient, expects them to heal the lungs." Is that passage correct, that for local disease in the lungs it is no cure to pour cod-liver oil and tonics into the stomach, for they never reach the part affected?—The statement is correct, but the inference from it is equally incorrect.

The Lord Chief Justice. Do cod-liver oil and tonics ever reach the part affected when taken into the stomach?—We have every evidence that cod-liver oil and other medicines taken by the stomach do reach the lungs, and every part where the blood can circulate.

They do not reach it in the shape of cod-liver oil?—Yes, I have no reason to doubt it.

Do not they reach the lungs in the shape of blood after passing into chyle through the different processes and so into the blood?—We have no reason to believe that cod-liver oil is converted into blood itself, but there is every reason to believe that it is converted into chyme, and that it supplies the oily matter in the chyme and passes into the blood, and, as blood, is capable of affecting all the tissues of the body.

I understand that though it is not converted into blood, it is carried in the shape of oily matter with the blood to all the tissues in the system?—Yes, it may assist in the formation of the blood as one nutritive material, but it is more than that.

Then am I to understand you to say that you think its beneficial action on the lungs is by the oily matter or the cod-liver oil being brought into *immediate contact with the diseased part of the lungs*?—I believe so, my lord, as one mode of action, but I believe it acts in various ways as a nutrient, as well as by direct application.

In other words, if you could get at the lungs, you think it would be beneficial to apply cod-liver oil locally to the sore place?—That I doubt, my lord—in the way of oil; it is through the blood which pervades all parts and supports the nourishment of the body. The deposition of the tubercle, the formation of sores in consequence of the presence of tubercle, all depends on the blood circulating, and the healing of those sores, the absorption of the tubercles, depends likewise on the circulation of the blood in and around the parts; and therefore we may consider that, through the circulation of the blood, we may get at the tissues, and get at once to the tubercles more effectually than through the air passages. I hope it will be observed that this is going beyond matters of actual fact—this is *opinion*.

The Lord Chief Justice. You must not forget you are also dealing with another man's opinion.

The question is, not whether medicines administered through

the stomach reach the blood or not, but whether so administered they afford the greatest measure of relief that is possible. It is simply absurd to say that they exert any *direct* action on the membrane lining the internal surface of the air-tubes. Dr. Williams thinks they do act *directly* on the tubercle. If they do, then they must pass from the blood-vessels into the air-passages. Cod-liver oil might thus *grease* the tubercles; and, since Gulliver and others have discovered that tubercles already contain too much oil, possibly this accounts for the increase of mortality, from this disease, since the introduction of Cod-liver oil! These differences are best settled by an appeal to statistics. Do *fewer* people die from diseases of the lungs in England now, than before the introduction of Cod-liver oil? If not, then all the *theory* and *dogma* in the world, will not prove that it has done any good. If *more* people die, then the presumption is fair that it has done *harm* rather than good. We shall presently see how this is.

Mr. Quain. Now another passage, if you please, I am going to call attention to, at page 36, in Roman numerals:—"On the progress of the vapour treatment in this country." I will read the whole passage. It begins:—"If anything were necessary to establish the efficacy of the practice of administering medicines in vapour, in all diseases seated in the air-tubes, cells, or substance of the lungs, surely enough is furnished in the grateful acknowledgments of those who have tried and proved, in their own persons, the curative powers of this treatment. No man of unprejudiced mind can read the particulars of the cases so graphically detailed by Dr. Melville, who, in association with myself, for several years has had ample opportunity of observing the results of this method of practice, or the voluntary testimony of the accompanying letters from patients who have been restored to health; and yet resist the conclusion—that pulmonary complaints are curable, even after they have reached a confirmed stage, when treated by properly regulated and adapted inhalations of oxygenated and medicated vapours. I cannot but regard it as a contribution to medical science of inestimable importance to mankind, and one destined to exert a more beneficial influence on the practice of medicine than any discovery of modern times." That is the main passage; and then he goes on to say—"I do not hesitate to say that no such results as those detailed have ever before been effected in these diseases. All observation and experience, both in and out of the profession, prove the utter worthlessness of the common routine of fish oil, sedative cough mixtures, and acid tonics, administered through the stomach, on which medical men have hitherto entirely

relied. The people of this country, judging from the results of such practice, have come naturally to the conclusion that consumption is beyond the reach of medicine and human skill. They have seen many cases so treated, but never yet witnessed a well-marked cure. The truth is, the public has been led into error. Instead of failure having resulted from the inveterate hold of the disease upon the system, it has proceeded almost invariably from the irrational course of treatment pursued. It has proved more fatal than other organic diseases, only because the treatment pursued has been false, and contrary to the best established principles of medicine." Then, in the next page but one, page 39, there is this passage:—"Time, stern facts, and the pressure of public opinion, will gradually bring about this desirable medical reform. As young physicians go forth into the world, untrammelled in theory and uncommitted in practice, they will, one after another, abandon the treatment by the stomach altogether; and not only adopt, but ultimately carry to even a higher degree of excellence, the rational and successful system I have laboured to introduce." Now upon that passage, I want to ask you, have you, during the long time you have been in practice, been in the habit of using inhalations yourself?—Yes, I have.

The Lord Chief Justice. Since when? Throughout the whole course of your practice?—Very nearly.

Mr. Quain. For how many years?—I may clearly say for thirty years I have been in the habit of using inhalations.

I mean inhalations for throat, bronchial, or pulmonary affections?—Just so.

Have they been used to your knowledge by other physicians in their practice?—Yes, they have.

Have they been used at the Brompton Hospital for many years past?—Yes, they have.

Will you tell us what inhalation, speaking generally, without going into details, you used, and with what results?—Myself, you mean?

Yes, your own practice?—Inhalations most commonly employed are those containing creosote or carbolic acid, which is much the same thing, hemlock, or conium—I use the English term.

The Lord Chief Justice. It is the same thing; but one is the English term, and the other the Latin?—Yes. They also use stramonium and chloroform, and it is for the most part with the vapour of hot water.

With any instrument or without?—Formerly I tried instruments, but of late I have found it much more convenient to desire a patient to breathe over a jug, confining the vapour by a napkin round the face. I would mention iodine as an agent which I have formerly used a great deal of late.

Mr. Quain. Now, has this use of inhalation been common in your practice, and to the other physicians, to your knowledge, for many years?—It has been common in my own practice.

The Lord Chief Justice. You would hardly say it has been common with other physicians, has it?—I should say so.

Common?—I think so, certainly; within the last ten or fifteen years I have found a great many of my colleagues in the habit of prescribing inhalation.

If Dr. Williams has understood and employed inhalations “for thirty years,” how does it happen that he has not told us in any of his publications, either the kind or the strength of the inhalations he used? I do not find any detailed account of his experience of them as “valuable auxiliaries,” nor any formulæ for their preparation, in either *Williams on the Chest*, or *The Principles of Medicine*. Has he been keeping his knowledge a secret?

Mr. Quain. Now tell us what has been the result of your own experience as to the result of inhalation?—I have found inhalation a valuable auxiliary in the treatment of throat, bronchial, and pulmonary affections; but after having tried it to a great extent, and in various ways, I would place its importance as subordinate to that of medicines administered by the mouth—I mean swallowed.

The Lord Chief Justice. That is, medicines taken into the stomach?—Medicines taken into the stomach. May I give the reason for it?

Mr. Quain. Yes, by all means?—It appears to me that inhalations are transient in their effects. Although there is no reason to doubt that medicines so administered reach the air-passages of lungs, and even the blood, it is in such minute quantities that their effect is not enduring. Another difficulty in their use is, that it is hard to apportion their strength. Inhalations which even in some persons would produce little or no effect, in others, for the time being, produce a strong effect—stupefaction, giddiness, sickness, and other unpleasant symptoms. It requires great care, therefore, in the management; and, as I say, their effects are limited. May I exemplify this in one case—the case of asthma—the case in which inhalations may be supposed to be most effectual? I have prescribed various inhalations for asthma; but there is often a difficulty in the administration of inhalation; and, as I have said before, there is uncertainty in the result—that is, with the agent of stramonium—an agent, I believe, which Dr. Hunter employs—that has long been used for smoking and inhalation, and its effects are acknowledged in common use throughout the profession all over the world; but these effects are transient; whereas that same stramonium administered by the stomach is much more sure in its effects in relieving a fit of asthma.

I am glad to know that he *now* regards inhalation as a “valuable auxiliary” in the treatment of pulmonary diseases.

From his work on the Chest I was unable to discover what he employed, as he *prudently* abstained from saying one word in regard to treatment. Even in his *Principles of Medicine* there is not a sentence under the head of *Tubercular* disease [Consumption], to show that he ever administered inhalation as an *auxiliary* in the treatment of this disease. Still there is enough to satisfy the reader that he recognises the desirability of promoting the “*solution of the deposits, chiefly by the oxydating influence of the current of arterial blood.*” This, he tells us, “*naturally suggests the inquiry whether it is possible to aid this process of oxygenating the blood, more highly than can be done by the free respiration of pure air.*” “I have already suggested,” says he, “that it is *probable* that such agents as nitric and nitro-muriatic acids, and *chlorate* of potassa, may contribute to this object.” Again, “these *oxygenating medicines* also, sometimes produced *decided improvement in the general and local symptoms of scrofulous and tubercular diseases.*” (*Williams’ Principles*, p. 492). This, then, is why he gives *Acid Tonics*; and why, in my book, I said that the chief reliance was on Cod-liver oil, acid tonics, and cough mixtures. It is, therefore, plain that Dr. Williams thinks it necessary to *get more oxygen into the system*; and that the only difference between us is, that *I* administer it by the lungs, and *he* tries to do so by acid tonics through the stomach.

But if he has really been in the habit, “for thirty years,” of using inhalations “as valuable auxiliaries,” why, in the name of reason, did he not tell us so in his books? How was I to *know*, or to *suppose*, that he regarded them as valuable? How am I now to believe that he did so regard them; when, under the head of remedial measures, he does not even *mention* them? He tells us of “Cod-liver oil,” and “mercury rubbed into the skin,” and “acid tonics”—barely alludes to two or three other medicines, as of *possible* utility in some cases—and there *his* treatment ends!

He says, inhalations only enter the system in “very minute quantities.” This is true; and it is one of the chief merits of the inhalation practice. It does not fill the system with crude

drugs, and yet it produces much more powerful effects. "It is "surprising to witness" (says Dr. Carpenter) "the extraordinary "increase in potency, which many substances exhibit when "brought into relation with the blood in the gaseous form." So that a *little* medicine, *inhaled*, may do more good than a *large quantity swallowed*.

Then one question. Take the cases that we had yesterday of chronic catarrh, bronchitis, spasmodic asthma, catarrh of the nose, and affections of that description, chronic asthma—would you expect the inhalation of the drugs you have mentioned, or some of them, would be beneficial?—Yes, they would be beneficial to that limited extent.

And have such inhalations been prescribed for such complaints for many years within your knowledge?—Certainly.

Apropos of that, I will call your attention to a passage in page 116 of the book, a passage in *pari materia* with what I read at the beginning of the book. "From what I have said, it will be understood "that by inhalation is meant something more than the indiscriminate "employment of particular remedies. The physician who would "employ it with success must not only be possessed of the requisite "instruments, and understand thoroughly its principles, but also its "practical application in the several forms and stages of pulmonary "disease. At present few medical men can pretend to know more on "the subject than the possibility and safety of introducing medicines "into the lungs in a state of vapour." Is that a correct statement of the medical knowledge of the profession, that all you know about inhalation is the possibility of safely introducing medicines thus into the lungs?—No; we know a great deal more than that, and some of it to the disadvantage of inhalation. It is one of the expedients of which we have availed ourselves and tried to the utmost. We have tried it in a great variety of ways, and in many instances it has been found wanting; therefore we do not give it so high a position as Dr. Hunter does.

Now, let me call your attention to an important passage in page 113.

The Lord Chief Justice. He says you know that the physicians who would employ it with success, must not only be possessed of the requisite instruments, and understand thoroughly its principles; but also its practical application, and so forth. Now are you acquainted with the instrument which Dr. Hunter uses?—No, I am not; it has been impossible to discover what it is.

Mr. Quain. My Lord refers to the inhaler, or the instrument used for inhalation. Do you refer to the same thing?—Yes; I have not seen it.

The Lord Chief Justice. There is no statement in the book of what the instrument is?—No; and my knowledge is only derived from t

book, and from conversing with some patients who have been in Dr. Hunter's hands.

Have you ever tried inhalation by means of instruments?—Yes, I have.

But such as you have been acquainted with has not proved efficacious?—Less effectual than the simple mode I have mentioned, that of an open jug. In most apparatus, a tube is too small; it is adapted to the mouth only, the patient is fatigued, and the inhalation is less perfect; whereas, in an open jug, he breathes with both his mouth and nose, and can regulate the inhalation without inconvenience.

He breathes through the nose and the mouth at the same time?—Yes.

Whereas, by the instrument, they only pass the vapour through the mouth?—Through the mouth usually.

If I had no other reason for believing that Dr. Williams never had used inhalations properly, his own evidence abundantly furnishes it. Imagine a poor, suffering invalid, whose blood requires “oxygenating medicines” (p. 492), being set with his face over a “jug of hot liquid,” with a towel around the top of the jug and coming up above his nose; “*the patient then to inhale the vapour through both the nose and mouth, in the usual way!*” This may effectually keep in the steam, but it also keeps in and *compels the poor patient to breathe his own breath over and over again!* In the effort to get the medicated vapour, he is made to inhale the poisonous carbonic acid back again into his blood, which the Doctor thinks would of itself *produce Consumption*. After this admission, I am not surprised to learn that, in Dr. Williams's opinion, the patient requires “*oxygenating medicines.*” And this is all he knows about inhalation! No wonder he thinks less highly of it than I do. The surprise is, that the experiment did not kill every patient on whom it was tried. If he has done this for *thirty years*, no wonder the mortality has increased during that period!

Now, as Dr. Williams admitted that he had been questioning “some of my patients,” is it not strange that he did not ask them to show him my *inhaler*? They must have been on very confidential terms indeed; for he learned all the secrets of my practice, and came to know not only as much of it as I do, but, in his opinion, somewhat more. Then the instrument has been openly offered for sale in the chemists' shops all over London.

Was he blind that he could not see them? Can we believe that he ever tried to see them? It requires a marvellous amount of confidence and *credulity* to accept this statement, even though made under oath; and I confess I can only receive it *cum grano salis*.

Mr. Quain. Now, the passage to which I was about to call your attention was one in page 113, at the bottom of the page: "The medicines which it is necessary for the patient to inhale are of four kinds; first, expectorants, to expel the mucus; second, sedatives, to allay irritation; third, astringents, to diminish secretion; and, fourth, alteratives to change the action of the diseased membrane. These, by acting directly on the parts affected, not only promote the cure of the disease, but also from the commencement add greatly to the comfort of the patient by relieving his more troublesome symptoms." Having called attention to that, I ask this question: Does that give any sufficient information to an educated medical man, as to what expectorants, what sedatives, what astringents, or what alteratives should be used; in what proportion they should be used; or is it sufficient to guide them in adopting Dr. Hunter's treatment?—I should not be able to act upon it in any case. It is the most vague, general statement that can be possibly made; and the very terms—expectorants, sedatives, astringents, and alteratives, apply to a vast number of agents, the selection of which must depend entirely on the judgment of the medical man.

And the proportion in which they should be used also?—Not only the proportions, but the selections. This is no indication whatever of the real nature of the treatment.

And I may take the opportunity of asking you this question on the book as a whole. Is there anything in the book as a whole which would sufficiently indicate to a medical man what Dr. Hunter's treatment is, so that he might adopt and follow it?—No.

The Lord Chief Justice. What you have said of that particular passage is characteristic of the whole work. You say it is so vague that it would give you no sufficient information as to what the treatment ought to be?—Precisely so.

It is only necessary to remind Dr. Williams that the definition of the term "*Alteratives*" given in the best medical dictionaries is, "Medicines which gradually alter or change the diseased action of the body or part affected;" and, therefore, all that a physician requires to know is what he wishes to *alter*. Is there too much secretion? Then he should try to alter that by inhaling medicines which tend to diminish secretion. Is the membranous lining of the air passages dry and irritable? If so, he naturally wishes to change that condition by soothing and

emollient inhalants. Is the blood *impure*, he must first find out in what the impurity consists, and then, like a scientific physician, try to alter it as quickly as possible.

It may be that Dr. Williams did not know this before; but if so, his medical education must have been far more imperfect than is generally supposed.* But why is that a fault in my book (mainly written to convey information to the general public) which was thought none in his, written for the profession? *He* does not even *classify* the remedies to be inhaled; and I have yet to learn that, in any book ever written by him, he mentions the doses and manner of use of "carbolic acid," "stramonium," or other medicines which, under oath, he tells us he has been making his patients inhale from *jugs of hot water* for the past thirty years!

Mr. Quain. Now take another part of the book, page 106. We have at the top of that page, after stating—

The Lord Chief Justice. Had not you better go back to the beginning of the passage in page 105: "When I am asked by those who come to 'consult me?'"

Mr. Quain. I am obliged. "When I am asked by those who come 'to consult me, what course I advise them to follow, my invariable answer is,—'You must place yourself under treatment by inhalation. We must reach the tubercles in the lungs, and the carbon in 'the blood, and we can never do that through the stomach.' Do you ask why we cannot do so through the stomach? Because there is only 'one remedy known which will neutralise carbon in the blood; and that 'remedy will only act when it is brought into relation with the blood in 'the gaseous form; that is to say, breathed or inhaled into the lungs. 'What is that remedy? It is oxygen, in such admixture with nitrogen, 'or atmospheric air, as shall best adapt it to the indications of the case. 'It was the want of oxygen which first laid the foundation of the 'disease, and now it must be administered in excess to bring back the 'blood to its normal vitality."

Now do you find in the book any directions sufficient to guide an educated medical man as to the administration of oxygen?—No, I find none. After reading this book I was utterly at a loss to know by what means Dr. Hunter administered oxygen. Up to the present moment, I have learned no facts which warrant the supposition that he ever administered oxygen by inhalation.

The Lord Chief Justice. What I understand him to say, or one of

* But what will the reader think when he reads Dr. Williams's definition of an "alterative" inhalant? See page 176 of this book, fifth paragraph from the bottom of the page. "Iodine has," &c.

the other medical gentlemen called, was that the oxygen was inhaled through the medium of chloric acid.

Mr. Quain. I was going to call attention to the prescription which Dr. Melville gave us, which I think consisted of $1\frac{1}{2}$ drachms of iodine, $1\frac{1}{2}$ drachms of chloroform, 6 drachms of chloric acid, and the rest of stramonium, making up a 6-oz. phial?—How used?

Used as an inhalant?

The Lord Chief Justice. Not the whole at one time?—With hot watery vapour?

Mr. Karstlake. Yes.

Mr. Stephens. One teaspoonful of that mixture was put into the inhaler with a proportion of hot water?—As far as my experience goes, that would not develop oxygen in a *free* state.

The Lord Chief Justice. Chloric acid and hot water?—Chloric acid, as decomposed, would be resolved into chlorine and oxygen; and if those are disengaged *freely*, the chlorine would act in so irritant a manner as to prevent the inhalation of the vapour.

The reader will observe that all this is based on the supposition that chloric acid is used to get "*free*" oxygen, or oxygen in a "*free state*," which is precisely the thing I do not expect or wish to do. There is nothing in my book about oxygen in a "*free state*," but oxygen combined with other agents. The frequent use of the term *free* by Dr. Williams could only spring from an entire misapprehension, or the *intention* to raise a *false issue*.

Mr. Quain. Do I understand you, is that when chloric acid is decomposed alone, and not in combination with those other things?—Yes, alone.

Dr. Melville said, I think, that he never gave it in that way?

The Lord Chief Justice. No?—If given in combination, such combination as has been described, the materials given with it would absorb both the oxygen and the chlorine. That, I may mention, is my first impression at the first statement. I have not learned this before, but there will be other more competent chemical authorities on that point.

The Lord Chief Justice. So far as your chemical knowledge goes, that would be the result?—Yes, that there would be no free oxygen inhaled.

If administered alone, the chlorine, when set free, would be a violent irritant of the lungs?—Yes, quite.

If these other things that we have heard of were in combination with it, then both the chlorine and the oxygen would be taken up with the other substances?—It would combine with them.

As he does not pretend to have ever used "*chloric acid*" in any form, everything he says in regard to it is simply his *theoretical opinion*. *Practically*, he does not know anything

about it. In my evidence, I stated that I used it as an “*oxy-genating inhalant*.” Now, it so happens, that in the form used by me it is not decomposed at all, but merely raised in vapour in the air, and in that form is carried into the lungs. It enters the lungs as an acid minutely divided, and mingled with the air. The air is merely the carrier. The decomposition takes place in the lungs, if anywhere, and as neither irritation nor any unpleasant effect has ever yet followed its use, all his theoretical speculation on that point is mere *moonshine*. The chemico-vital affinity of the oxygen for the blood is so much stronger than that of oxygen for chlorine, that, *theoretically*, we should look for decomposition to follow its inhalation in the form of fine mist. But, deny that this does follow, we have still the indisputable *local action on the lungs*—the fact that patients *recover under its use*—and the further fact that *no living soul in whom it has been used* ever experienced more than a slight sense of dryness in the fauces. Again, chloric acid is only *one of many* oxygenising compounds which I prescribe daily, and concerning which I have always been willing to inform such of my medical brethren as were really anxious to understand something of my practice.

Mr. Quain. And I suppose during the time of inhaling, atmospheric air is not inhaled to the same extent?—No; in proportion to the quantity of vapour infused, to that proportion is atmospheric air displaced; and that, I may mention, is one disadvantage of all inhalation. You cannot have it without having the air impaired.

If I understand you rightly, the man would breathe less oxygen than if he breathed common atmospheric air?

The Lord Chief Justice. That would depend on the supply in this mixture given to him.

Mr. Quain. You are excluded from the air you are inhaling; therefore it is, *pro tanto*, displaced.

The Lord Chief Justice. If he is right, that you cannot get any oxygen at all, you are taking in something which may or would be beneficial, but you are losing something which is really beneficial.

Mr. Quain. That is so quite, because the passage I call attention to is, “What is that remedy? It is oxygen in such admixture with “atmospheric air as shall best adapt it to the indications of the case.” What are the component parts of atmospheric air?—If I recollect rightly, I think 21 of oxygen, and the remaining parts of hydrogen—79 within a fraction.

It must have been a great tax upon Dr. Williams’s gravity to

be compelled to listen to his Lordship and the counsel for the defendant making out, to their mutual satisfaction, that inhalation was maintained not only without *oxygen*, but by means of some mysterious *aura* drawn up from my inhaling instrument, the patient being "excluded from the air!" How the doctor could sustain his part in the farce I cannot imagine. He has, however, a way of dipping his chin and hiding his emotions in his bosom, and no doubt his spirit on this occasion indulged in an internal roar!

Neither the Chief Justice nor the counsel evidently understood that oxygen is the *only* food of the lungs, and that whatever else you have, you must have that. You may increase the oxygen, you may give it pure, you may impregnate the air with remedial agents, and it will become the carrier of them into the lungs; but, whatever else you have, you must have the oxygen of the air, or pure oxygen, or some intermediate compound containing oxygen. Having settled, however, the point, that the mixture gave no oxygen, and that the patient was even excluded from getting the air, the defendant's counsel, naturally anxious not to disturb so satisfactory an impression, proposed to take up another part of the book.

Then, going on to a passage in page 14 of the book—I will begin at page 13, so as to read the whole passage—"Some years ago it became fashionable to treat this affection"—that is, granular sore throat—"by applying to the diseased parts a strong solution of nitrate of silver, or lunar caustic, with a probang and sponge. These applications occasion great suffering to the patient, and burn and disorder the mucous membrane, without a tenth part of the good attained by milder means. Where the larynx has become affected, it is both cruel and dangerous to force it into the delicate organ of the voice, designed by nature only for the reception of air, strong, irritating fluid caustics." Now the next passage is what I wish to call your attention to: "Fully one-half of the consumptive patients whose cases have come under my care, were previously treated for disease of the throat by these caustic applications; and, as they assure me, without any permanent benefit. Indeed, many of them do not hesitate to attribute the disease of their lungs to this practice of burning their throats." Now, a question about that. These caustic applications for granular sore throat, ending in or causing consumption, what is your opinion about that?—I do not see any possibility of any such result following. I cannot see the connection between

caustic applied to the throat, and the development of tubercles in the lungs.

It is surprising what very great discrepancies there are in the opinions of doctors. Their disagreements have grown into a proverb, and you could not pick up an urchin ten years old in the streets of London who had not heard that "doctors differ." We have heard Dr. Williams, let us now hear what Professor HUGHES BENNETT says—

"A gentleman supposed himself to be labouring under sore throat, and his medical practitioner had, during a period of three years, been from time to time sponging the fauces and glottis with a solution of nitrate of silver. No suspicion existed in either party that the lungs were affected. Careful examination convinced me that a large dry cavity existed at the apex of both lungs, and that the great vocal resonance and sound on percussion had misled his professional attendant. This case terminated fatally by hæmoptysis a few months after I saw him, when the correctness of my diagnosis was confirmed by dissection. Since the local treatment of the larynx has been generally practised, cases where pulmonary phthisis are mistaken for chronic laryngitis, and *vice versa*, are by no means uncommon" (p. 67).

Dr. HORACE GREEN, in his work on diseases of the throat, says—

"That peculiar affection of the throat, which, under the appellations of 'Bronchitis,' 'chronic laryngitis,' 'clergymen's sore throat,' &c., has occurred, especially during the last ten or fifteen years, so frequently among public speakers, and others, consists. . . . in a diseased condition of the glandular follicles of the mucous membrane of the throat, larynx, and tracheæ.

"The structural changes to which the mucous follicles of the throat and air passages are liable, are *inflammation*, *induration*, or in a deposition of *tuberculous* matter in the follicles themselves. . . . It may accompany, or be consecutive to, other affections of the air passages, and to exist with *laryngitis*, *Bronchitis*, or with *Consumption*" (p. 43).

"As this disease, in its advanced stage, seems to be *constantly attended* with a secretion, either within the mucous follicles, of a peculiar concrete substance, resembling *tubercle*; or, with an infiltration of this tuberculous matter in the sub-mucous cellular tissues, it may with propriety be denominated *tubercular sore throat*" (p. 49).

All the late authors have called special attention to "*follicular disease*" in connection with Consumption. Dr. GREEN gives numerous cases showing not only its tendency to the lungs, but its *scrofulous* character. Dr. EDWARD SMITH refers to it as a peculiar form of throat found in Consumptive cases. Dr. Williams seems to know nothing about it, but surely his

want of *observation* or *negligence* in examining his cases is no fault of mine. Enough for my vindication is the fact that it exists as a frequent forerunner or attendant on tubercular Consumption of the lungs.

Now, in the very same page, there is an important passage about elongated uvula, which I want to call your attention to—the end of page 14; the paragraph begins, “We not unfrequently find the uvula, or little pendulum, commonly called the palate, so greatly increased that it hangs down upon the root of the tongue, and keeps up a constant irritation. It may also be increased in thickness, and indurated, although such is not usually the case. Elongation is produced by repeated attacks of cold, and is often an attendant on catarrh and granular sore throat. The amount of injury and annoyance this condition of the uvula will sometimes occasion, is truly surprising. I have seen a strong, healthy man get an obstinate, harassing cough, and lose twenty pounds in weight in the course of a few months from no other cause. The point of the uvula sometimes introduces itself into the entrance of the windpipe, causing great difficulty of breathing, and loss of tone and power in the voice, with a sense of suffocation. It occasions in many persons attacks of nightmare.” The next passage is what I want to call your special attention to: “When neglected, it commonly ends in permanent injury to the lungs, and often develops tubercular consumption?”—Well, that is contrary to my experience. I never knew an instance in point.

You never knew a case of it?—Never of elongated uvula terminating in consumption. Of course, elongated uvula may occur to tubercular subjects; that is a different thing.

We shall now see what other and greater authorities say on this subject.

Professor GROSS says—

“The uvula, from debility, inflammation, and other causes, is liable to chronic enlargement, especially elongation. . . . Very disagreeable effects may be produced by an elongated uvula. . . . The most common effects, however, are obstinate and protracted cough, with frequent desire to clear the throat, titillation of the fauces, dryness of the mucous membrane, and a feeling of constriction and frequent hawking. *When the affection continues long, tubercles sometimes form in the lungs, and the patient ultimately dies under all the symptoms of confirmed Consumption*” (p. 576).

Dr. WM. STOKES says—

“ . . . he has seen cases presenting all the usual symptoms of Consumption, except the physical signs such as cough, puriform and bloody expectoration, hectic emaciation and quick pulse, which were produced by relaxation and elongation of the uvula, and in the treatment of which the ordinary

"means either altogether failed, or were only partially successful."—(Green on "Bronchitis," p. 100—Stokes on the "Chest," pp. 230-1.)

I might give a dozen different authors in proof of the direct connection existing between the throat and the lungs, but these are sufficient to show that I had both authority and experience for all that I said on the importance of attending to the throat and uvula.

Just to return for a moment to one subject. I have a question to ask you. Have you ever turned your attention to the administration of oxygen, I mean free oxygen?—Very rarely. I have prescribed the inhalation of oxygen for patients, and the only way I know it can be administered is as a gas, by means of a gasometer. I have done that in a few cases.

The Lord Chief Justice. With what result?—No satisfactory result, in most instances; rather negative in most cases.

Not sufficiently satisfactory to lead you to adopt it generally?—It is a difficult and rather a troublesome process to the patient; and, unless it were followed by large benefit, it is not one that can be easily persevered in.

Mr. Quain. Are there reasons, in your judgment, which might render the administration of it either dangerous or pernicious?—In certain cases of pulmonary disease, I think the inhalation of oxygen might be injurious. *It increases the stimulating property of the air.*

The Lord Chief Justice. And the effect of that stimulating property, is what?—As many inflammatory conditions are comprised in the state of consumption—consumption includes many inflammatory affections; for example, bronchitis and congestion of the lungs, and a tendency to hæmorrhage—I should infer the administration of oxygen would in many cases prove injurious.

When the reader arrives at the evidence of the chemist, he will learn that it produces no *sensible* effect whatever. It is not that Dr. Williams ever saw injury or increase of excitement from it, but he "*infers.*" Why does he not say when, and where, and how he ever tried it, and then state the effects observed? He has no right to *infer* what, if he knew anything whatever of the effects of oxygen, he would know *could not* be true.

Dr. Williams does not state, in his *Principles of Medicine* anything which would lead me to believe that he had ever administered oxygen as a gas. He speaks of it as a *possible* means of purifying the blood. There is not a word of explanation as to how it is to be generated, or how administered, or any statement of his *experience* in its administration. He says—

"Experience has not yet furnished us with any other means of arterialising" [purifying] "the blood than the process of respiration. This, he tells us, may "be carried on artificially, as by inflating the lungs," or by "electricity applied to "the muscles of respiration," or "it may be helped by *bronchotomy*" [cutting an "opening into the bronchia], or "by chemical means, *such as the supply of pure "oxygen or nitrous oxide.*" "Whether the internal administration," or "the "injection into the veins of saline and other matters containing much oxygen in "loose combination, such as the chlorates, nitrates, and some peroxides, may not "be made to aid, in some degree, in compensating for *defective respiration* is "worthy of consideration, and more extensive trial than it has yet received. If "these substances thus exhibited could furnish oxygen to the blood, they would "yet leave unaccomplished the other office of respiration—the removal of carbonic acid" (pp. 187-8.)

Now this is all he says about the inhalation or use of oxygen. Does it convey the idea that he had even used it? As for cutting a hole into the bronchi, I will venture to say he has not found many patients willing to submit to that; and any that might have done so, I fear do not now *live* to inform us how very beneficial it was! He evidently thinks it most important to get oxygen, or nitrous oxide, which is much the same, into the blood; but he does not tell us that he had really introduced it. Even the *chlorates*, &c., which contain "much oxygen in loose combination," he merely suggests as *likely* to overcome the imperfect respiration, but does not pretend that he has *tried* them, or that he *knows* what effects would follow their use. Is this not enough to show us that all his pretended knowledge is mere *theory*? From reading his books I certainly arrive at the conclusion that, when he wrote them, he thought there was too much *carbon* in the system in Consumption, and believed that *oxygen* might be artificially introduced for its removal; but that he was completely without experience as to the best means of doing this, that he had never tried it, and merely threw out the suggestion that, if attainable, it might be productive of great good.

Mr. Quain. Now, I would call your attention, in conclusion, to a passage in the first edition, by Dr. Macgregor, in which he says, "The "almost hopeless apathy of the medical profession, and the despair of "the public mind, on the successful treatment of consumption, must "have aroused the attention of the most casual observer." Now, let me ask you; during all the time you have been in practice, so far from

all the medical profession being affected with hopeless apathy on the subject of consumption, has every attention been paid to it in every possible way?—Certainly it has. I know no disease in the whole circle of diseases to which human flesh is prone, which has been studied with more attention, studied with more care, or which has been more popular with the medical profession than pulmonary consumption.

And, speaking generally, without going into numbers, which we will go into in a moment, comparing the beginning of your professional life with the beginning of the present time, has the mortality from consumption increased or decreased?—So far as my own personal knowledge goes, I should distinctly say that the mortality has diminished.

The Lord Chief Justice. From consumption?—From consumption. May I conclude my sentence on that same point? The mortality stated in this book does not appear to me to be at all an accurate statement; it is an exaggeration of the mortality; but it may be stated that in the time of Laennec and Louis—Laennec, who was the original teacher on the subject, and Louis, who still lives—the average duration of consumption was rated at two years. I think I may state, as a result of my own experience in these cases—I have had an opportunity of watching from first to last—the average duration may be put down as five years, so much diminution in the mortality has taken place.

The two years have increased to five?—Yes.

Where a patient would have lived two years in the time you are speaking of, the patient now lives five years?—Yes, on the average.

As I shall presently submit these matters to the statistical test (after Dr. Williams has sufficiently committed himself to be unable to escape), I will only here remark that whatever may be the case with other members of the profession, Dr. Williams has shown amazing *apathy* in never having detected the connection existing between “*follicular disease*” and Consumption, and between elongated uvula and Consumption, when it has been noticed and pointed out by numerous observers. Then it will be found that he is utterly at sea in his statistical calculations. With reference to the duration of life being increased, that is *apparent*, but it is not *real*. The average duration is a very different thing from the chance of life in this disease. A few patients linger on for many years, but the *majority* die within *eighteen months* from the commencement. “*Of 215 fatal cases at the Hospital for Consumption, more than half the entire number of cases, omitting the doubtful ones, were fatal within a period of eighteen months (123 to 78)*” (Ansell, p. 405). It is

all very well to amuse the public with such terms as "*average duration*," but when a man falls ill of Consumption he wants to know what is the probable duration of his life. It is not more than fourteen months. Then, regarding the *average* or *mean* duration, Sir Thomas Watson states it at about "*two years*," in the edition of his lectures published 1857.

Cross-examined by MR. COLERIDGE.

Is this true, do you think? "This fearful scourge, this insidious foe to the human race, speeds on its fatal course, slaying its tens of thousands annually, notwithstanding the elaborate investigations of the most astute pathologists all over the world?" Do you think it is true that the most astute pathologists all over the world are making investigations into the matter?—Yes.

Do you think that "such observers as Laennec, Andral, Louis, Bayle, and Walshe, with a host of others, have exhausted their microscopic researches after death, in tracing out the ravages which disease has caused in the delicate organisation of the lungs, or turned their ears to discover the manifest aberrations from healthy respiration during life?" Is that true?—Yes.

Are you aware that those are the two successive sentences to the sentence you say is an unfair one—that those are the two next sentences to the sentence you read?

The Lord Chief Justice. What page is it?

Mr. Coleridge. The very page, my lord, in the preface to the first edition.

Are those the two next sentences of the page which you have read from?—That preface does not appear to be in the copy which I have been looking at.

Excuse me?—I have it now.

Then am I right in saying that those are the two next sentences?—In this preface?

In that preface?—Yes.

Do you think it fair to represent that the writer of that preface speaks of the medical profession as being apathetic without any qualification?—That, unquestionably, so far modifies it, that it seems in one part to contradict the other.

Had you read the preface?—I had read the preface.

And did you deliberately and intentionally stop short at the first sentence?—I answered the question which was applied to me.

"However science may have profited by these investigations, the great aim of all their labours remains unattained." Is that true?—No; I do not consider that it is altogether unattained.

Do you consider that consumption is curable or not?—I unquestionably consider it curable in certain forms and in certain degrees.

The Lord Chief Justice. While you are still upon that, just ex-

plain what it means, otherwise the answer is very imperfect?—I consider consumption curable in certain forms and degrees in the incipient stage, and even beyond the incipient stage, where the degree is limited.

Mr. Coleridge. In the incipient stage, where it has gone beyond a certain point, you consider it incurable?—I have just said, even after the first stage, where the disease is limited, the disease is curable ; but when it has got beyond that, it is incurable.

Beyond what?—The limited stage, to a limited extent. If it is to any extent, I consider it incurable.

Then it is more important to catch it in the incipient stage than when it has gone beyond?—Decidedly.

You can hardly begin too soon, I suppose, can you?—No ; provided the disease exists.

And you can hardly begin too soon, if you apprehend the disease exists—if you suspect it?—Certainly not.

Now were you ever one of the Medical Council?—Under the Government?

Yes?—No.

I think you were President of the College for a short time, were you not?—No.

You have been on the Council of the College?—Yes, I have—the College of Physicians you mean?

Yes. Now let us see some of these passages—just turn to page 7, —“All observation and experience unite to prove that the root of this malady is in the lungs, and that tubercles are but the fruit of imperfect respiration. If we would cure it, we must attack it here ; we must remove the mucus which obstructs the tubes, and restore the purity of the blood by acting upon it through the air-cells.” That you said in chief was as much opposed to the truth as a statement could be. Do you adhere to that?—That is the first part—those three lines given to me. Those are distinct propositions.

Just give me what you say is as much opposed to the truth as can be?—The three first lines—“All observation and experience unite to prove that the root of this malady is in the lungs, and that tubercles are but the fruit of imperfect respiration.”

Just attend—“Tuberculous and scrofulous deposits, then, whether “in the offspring of scrofulous consumptive parents, or others, are “the invariable results of insufficient imperfect respiratory function and re-breathed air.” Is that as much opposed to truth as a statement can be?—Where is that?

Never mind where it is—Is that as much opposed to truth as a statement can be? I will read it again—“Tuberculous and scrofulous deposits, whether the offspring, &c., &c.” Is that a statement as much opposed to truth as a statement can be?—It is not so strongly stated or so exclusively as the statement that has been quoted.

Draw my attention to the distinction between this, that the

malady is in the lungs, and the tubercles are but the fruit of imperfect respiration, and this statement that "tubercles," &c., &c. [down to] "re-breathed air"—draw my unlettered mind to the distinction between the two, that makes one as contrary to the truth as a statement can be, and the other not quite so strong. First of all, is the statement contrary to truth?—I should say it is rather an over-statement of the fact, than so opposed to the truth as this statement.

What is the distinction?—If I had the passage, I could point it out better.

Just see whether you can recollect?—I want the new passage which you put before me.

I will read it.—Scrofulous deposits, whether the offspring, &c., &c., down to "re-breathed air."—Re-breathed air; just so. Yes, that implies a different result from the former, inasmuch as the re-breathed air is not deficient in oxygen only; but impure from other causes.

Then the distinction, I will take it, which makes one as contrary to the truth as a statement can be, and the other only not so strong, is, that one speaks of imperfect respiration and does not express re-breathed air, and the other does. Is that it?—Yes.

This is a mere play upon words, since he has said that breathing impure air will produce tubercles; and impure air, in the sense used by me, *i.e.* pre-breathed air, is only air poisoned by the carbonic acid of the breath; and as this is one of the causes of imperfect respiration enumerated by me, it is an attempt to make a distinction where none exists. Why is respiration imperfect in a close room? Because the air soon becomes deficient in oxygen, and contaminated by carbonic acid expelled with the patient's breath. How does it act? It acts by preventing the lungs from receiving sufficient oxygen to expel the carbon of the system, then the function of the lungs necessarily becomes impaired, and the blood imperfectly purified. This is imperfect respiration, and this Dr. Williams *admits* will produce tubercles. The paragraph quoted from his book (p. 53) will show that he was merely quibbling to hide the truth, mislead the Judge and Jury, and create a false impression.

Now, I will tell you where it comes from—it comes from a work of Dr. Henry McCormack, on Consumption, as engendered from re-breathed air, &c., &c., published by Longmans in 1865. Now let me go on and see what his notions are.

The Lord Chief Justice. What is the meaning of tuberculous

scrofulous deposit?—Deposit of tuberculous matter or scrofulous matter—they resemble each other so closely that some pathologists identify them, and others make a distinction. The scrofulous deposit may take place to a large extent without the formation of positive tubercles. Tubercle is where scrofulous matter occurs in a separate and distinct form, and they are both instances of it.

That writer, whoever he is, speaks of the two as the same, or combined, I am sure I do not know which. He speaks of tuberculous scrofulous deposits—does not that mean one and the same thing?—I presume it does.

Mr. Coleridge. Is Dr. Cotton an authority?—Yes.

I see he says that consumption and scrofula are identical.—Yes, that is the common opinion.

The Lord Chief Justice. What do you say is the common opinion? What does it mean, that scrofula and consumption are the same?—Scrofula is a term more applied to the constitutional disposition of persons, who are said to be scrofulous when liable to certain unhealthy depositions in the lymphatic glands, glands of the neck—to ulceration and disease of the bones. Internal scrofula affecting the organs is not to be distinguished from tubercles.

Then it is the result of a scrofulous condition?—So I believe myself. I believe that is the true view of the case.

Mr. Coleridge. Then we understand the distinction between those two statements. If I understand you correctly, you say that there are many classes of cases in which there may be an imperfect respiration, and yet not consumption?—Yes.

But is it true that in all cases of consumption there is imperfect respiration?—There is no evidence that imperfect respiration precedes the deposition of tubercle as a constant thing—as a constant antecedent.

I am glad to get a definite statement like this, for then I have a tangible fact, not a *shadow*. He says that there is no evidence that imperfect respiration precedes the deposit of tubercle. There he made a very palpable mistake. Imperfect respiration produces Consumption in all caged animals, and in all persons whose occupation is sedentary, and carried on in close rooms. Without the free circulation of air, respiration is always imperfect, in a greater or less degree.

Dr. EDWARD SMITH, one of his own colleagues at the Brompton Hospital, says—

“The respiration is shorter, shallower, feebler, and perhaps quicker” (p. 99).

Again—

“The association of shallowness and feebleness of respiration is seen in the

“most marked degree in tailors and shoemakers, clerks, and others, who follow very sedentary occupations, and sit with the chest bent forward. In such persons the act of respiration is at all times defective, and they are known to be very prone to the recurrence of Consumption” (Phthisis). (P. 103).

It is possible that Dr. Williams may try to escape from this, by saying whenever the respiration is imperfect there *may* be other causes operating, or there may have been some condition of the blood antecedent to the shortness of breath. Can he prove that such was the case? No, he cannot do that, but he can *assume* it! It is enough to know that if you confine 100 persons to a sedentary employment, without active exercise, as type-setters, about 44 per cent. of them will die of Consumption. If you confine them to the same rooms, but with the addition of exercise, as *pressmen*, only $31\frac{1}{2}$ per cent. will die (this exercise favouring the freer expansion of the lungs); while, if you allow them free exercise out of doors, only 25 per cent. will die.—Williams’s *Principles of Medicine*, p. 54.

These are the practical every-day facts of life, and Dr. Williams cannot deny it. He may attempt to explain it away, or try to make it tally with some pet theory of his own; but the naked facts remain; and for anything we do, or can know, the greater or less imperfection of respiration is the sole cause of the difference in the mortality in those instances.

Before I leave this point, I must call the reader’s attention to the quotation I have just made from Dr. Williams’s book. What does *he* mean by saying that 44 per cent. of *type-setters*, $31\frac{1}{2}$ per cent. of *pressmen*, and *one-fourth* of healthy out-door labourers, die of Consumption? I have been libelled out of reputation for saying much less; may he say one-fourth of out-door labourers die of Consumption, and I not? I call the Lord Chief Justice’s attention to this point; and refer him to page 54, Williams’s *Principles*, for the details.

Is it very usual?—In some instances it is said that impure air promotes the development of consumption; but instances occur where no obvious interruption to the functions of respiration has occurred, and yet tubercles are deposited.

The Lord Chief Justice. Your question relates to what happens a little later?

Mr. Coleridge. Yes.

Is not every case of chronic consumption attended with imperfect respiration?

The Lord Chief Justice. That is, when consumption has actually begun?—Yes, unquestionably it is so.

The Lord Chief Justice. When the action of the lungs is interfered with, you get imperfect respiration?—Certainly.

Mr. Coleridge. Therefore, although it may not be correct to say there is nothing else which is the cause or characteristic of consumption, it is true that this is the characteristic of consumption?—Observe, I said that, by itself, imperfect respiration cannot produce consumption.

The Lord Chief Justice. That is attributing to it a sense it may or may not convey; but if you have impaired respiration you may have disease of the lung?

Mr. Coleridge. Imperfect respiration may, and very often does, produce consumption?—In persons predisposed, it is an additional cause.

There may be cases of imperfect respiration where there is no consumption?—Just so.

But, as I understood you, in the great majority of cases there is imperfect respiration?—Do you mean before the consumption is developed?

Yes?—I do not know whether you can go so far as that.

How far can you go?—I cannot state with regard to the number of particular cases.

Give us a notion?—We often find instances of consumption being developed, without any antecedent history of obstruction to the respiration.

That you are aware of?—That I am aware of, or that can be traced by history.

All this is mere quibble, calculated to obscure the truth, and well designed to create false and injurious impressions. At first he said, the theory that "*imperfect respiration* is the cause of Consumption" "was as much opposed to the truth as any statement could be." Now, he admits that imperfect respiration might be one of the causes. "Insufficiency of oxygen" might lead to that deteriorated state of the system which results in Consumption. He thinks that there may be other causes, but he cannot prove that any combination of *other causes* ever yet produced a case of Consumption without *imperfect respiration being also present*.

If I understand you, you say it is an utter mistake to say that the

tuberculous deposit results from the want of proper consumption and passing off of the carbon?—Yes.

That is a total mistake?—Carbon does not pass off as carbon.

I do not say so; but is it a total mistake to say that tuberculous deposit is the result of imperfectly oxygenated blood?—Yes.

A total mistake?—A total mistake to say that it is the only condition.

The Lord Chief Justice. He is quite right.

Mr. Coleridge. Do I understand you to say that it is an entire mistake?

Mr. Karlake. Take the passage.

Mr. Coleridge. Well, I will take the passage, at page 90, "Tubercles are the consequence of imperfect oxygenation of the blood; their base is carbon. They may be inherited, but are generally acquired. They are produced equally in mankind and in brutes, by whatever diminishes the purity of the air or the freedom of its admission into the lungs. The body wastes in this disease, because the quantity of chyle assimilated is regulated to the weight of a grain by the quantity of oxygen supplied to the lungs. The seat of consumption is always in the lungs." I do not know that that was objected to; and "I have said that the blood is always impure in consumption. What is the nature of the impurity? Chemistry demonstrates that the oxygen is always diminished, and carbon always in excess. Carbon being a poison most inimical to life, I regard it as the true cause of tubercle. What does chemistry tell us of the essential nature of tubercle? The analysis of Scherer proves that fifty-four per cent. of it is pure carbon, and the remaining forty-six per cent. made up of the elements of disintegrated tissues of the lungs in which it is deposited." Now, I understand you to say that is an entire mistake?—An entire mistake with regard to the analysis of Scherer.

I take that first. If I understand you, the mistake consists in this: that whereas tubercle consists of carbon, and oxygen, and nitrogen, and hydrogen, the three latter ingredients go to make up the disintegrated tissue which is not carbon. Do you follow me?—Yes; it is an odd way of putting it.

I put it as plainly as I can, that the three things not carbon go to form that portion of the disintegrated tissue which is not carbon.—Yes; but remember that disintegrated tissue is one thing and tubercle is another. When we speak of the analysis of tubercle, speaking of the 54 per cent., you have to finish by what makes up the remainder of the tubercle.

Now, what does make up tubercle, besides the 54 per cent. of carbon?—Oxygen, hydrogen, and azote.

Nitrogen?—Nitrogen.

That is exactly what I said?—Yes; but not of disintegrated tissue.

Is tubercle made up of 54 per cent. carbon, and 46 of something which is not carbon?—It is.

Is that 46 per cent. which is not carbon, nitrogen, hydrogen, and oxygen?—Certainly.

Then what is the mistake?

The Lord Chief Justice. That you represent 54 per cent. of carbon, and the rest as disintegrated tissue; whereas the whole is disintegrated tissue; whether you call it so or not, it is so, and so much is carbon, and the rest nitrogen, hydrogen, and oxygen.

Mr. Coleridge. That is the difference, is it?

Now, you say carbon being the base, and carbon being the essential quality of it, is a mistake?—The excess of carbon.

Mr. Coleridge then read the following extract from Dr. MacCormac's book to show the identity of his views with mine on this point:—

"Hence the carbon is retained unoxymized, in other words, is not discharged or sufficiently discharged from the blood, and, finding no adequate outlet, being neither burnt off in the lungs nor expended in the tissues, is deposited mainly as a *hydro-carbon* in the lungs and other organs under the form of the body known by the designation of tubercle" (p. 113).

Mr. Coleridge. Is that quite incorrect?—That is quite incorrect.

That is quite incorrect, is it?—It is quite incorrect.

The Lord Chief Justice. Will you read that passage again?

Mr. Coleridge. "Hence the carbon is retained unoxymized," that is, from the imperfectly respired organ. It goes on [read down to the words "designation of tubercle"]. That, in your judgment, is quite incorrect?—Quite incorrect.

"The effete carbonaceous waste, in consequence of a vice of respiration, is not burnt off, or sufficiently burnt off. What then becomes of this unconsumed waste? The reply is, that it constitutes the foreign body termed tubercle, a body which inevitably forms when respiration, or rather the respiratory nism, is continued beyond a certain period in a more or less pre-respired effete atmosphere. Owing to the presence of the dead foreign matter termed tubercle, a morbid action is afterwards unavoidably set up, and hectic, wasting, and death are the inevitable result."—(Dr. MacCormac, p. 113.)

That is also a mistake, is it?—I cannot agree to it.

And a man is an ignorant person who says so?—It is an erroneous theory, whatever he may be; it is a theory which is inconsistent with our knowledge.

"The composition and constitution of tubercle, from whatever part of the body derived, at whatever age, and whatever be the animated being which is the subject of it, is essentially the same. It consists mainly of a hydrocarbon, and in case of absolescence and calcification of certain added mineral waste, circumstances of great

"moment in respect of arriving at a more definite knowledge of the nature, constitution, and ultimate disposal of tubercle. There is tubercle proper, deposited, we shall say, in the compound as many nucleated cells, as described by Virchow and Van der Kolk; and then there is the adventitious fatty matter so often incorporate with it. But this fat itself is almost a pure hydrocarbon, and may, perhaps, be also looked upon as a form of the unoxidised metamorphic waste. It abounds so in tuberculous liver as by Louis and others to be termed fatty degeneration of that organ. The fatty is coupled with the tuberculous matter, and is freely yielded on compression between folds of bibulous paper. Guillot, moreover, asserts that the dried parenchyma of tuberculous lungs contains from forty to fifty per cent. of fat associated with tubercle. It, is, however, stated by Scherer that crude pulmonary tubercle divested of foreign ingredients, and containing little fat extractive, yielded in one hundred parts, fractions omitted, fifty-four parts of carbon, seven of hydrogen, of nitrogen seventeen, and of oxygen twenty-one. Tubercle, then, is not a fixed or definite compound. But that carbon, waste carbon at any rate, is the solid predominant ingredient, is shown by direct analysis as well as by the concurrent testimony of all observers. "Nothing can possibly be more conclusive." (Dr. MacCormac, p. 107-8.) Do you agree with that?—No, I do not agree. I think it carries, on the face of it, its own contradiction. Scherer's analysis proves that tubercle contains only 54 per cent. of carbon, and that is the state in which tubercle is first formed. It is quite true that tubercles, when they get old, undergo what may be called fatty degeneration; but that is not tubercle in its original stage; and we have to deal in consumption with tubercle as it is at first, as described by Scherer, as containing only 54 per cent. of carbon, which is positively less than is contained by the other constituents of the blood.

Are you right about that? I think 48 is the proportion of carbon in the blood?—I believe all the constituents of the blood, fibrine and albumen, contain more.

What is the proportion in the whole blood?—I cannot quite recollect at this moment.

Forty-eight is it not?—Forty-eight in the thousand, I suppose you mean? That is quite out of all proportion; but there are chemists here who can tell you those chemical proportions. I do not profess to remember.

The composition of so important a fluid as *the human blood* should have been known to so eminent a physician. In behalf of the profession I protest against any reflections being cast upon us as a body. The case, I hope, is exceptional. What! not know the composition of the blood! It must have made the barristers feel queer to know that a distinguished physician

and lecturer on medicine, a stickler for "*taint*," and "*impure blood*," and *blood diseases*, when upon the witness-stand could not even give the composition of healthy blood. Perhaps since *blood-letting* has gone out, the blood is regarded by Dr. Williams as of no consequence !

The Lord Chief Justice. What book is that you have been reading from ?

Mr. Coleridge. It is Dr. MacCormack's work, pages 107 to 113, the edition of 1865.

Now, tell me, do you really mean to commit yourself to this : that the imperfect, what Dr. MacCormack calls "burning off of carbon" by the want of oxygen, is not one considerable cause of the making of tubercle—do you really mean to say that?—I think imperfect respiration is one of the imperfections of function which may contribute to degrade the system. Tubercle is a degradation of the proper material from which the body is nourished ; and anything which injures the functions of the body, is capable of leading to the development of tubercle. If breathing is interrupted, that is one of the functions, and promotes the formation of tubercle ; the perfect action of the heart, the inhalation, or the secretions—the purifying of the blood in other ways ; but what I say is, it is not the exclusion of oxygen, or the respiration of oxygen, that really can produce tubercle.

I do not say that ; but does not the carbon require to be burned off, as Dr. MacCormack expresses it, by the oxygen taken in by inspiration?—Certainly.

And if it remains in the lung unconsumed, it is one considerable cause?—Yes ; but that would not produce tubercle.

What would it do?—It would do mischief.

What kind of mischief?—If in excess, it would produce what is called asphyxia ; if it is stopped altogether, the blood becomes poisoned by the absence of oxygen and the presence of carbonic acid.

Then, do I understand you that taking an inefficient quantity of oxygen into the lungs is not, in your judgment, one cause of tubercle?—It may be one co-operating with others, or producing that deterioration of the system which leads to the formation of tubercle.

Is it, in your opinion, the chief cause?—I can scarcely say it is the chief cause.

Impure air was laid down by Baudelocque as the *only true cause* of Scrofula and Consumption. Respiration must always be imperfect when the air is impure. But they answer, "pre-breathed air," in the sense used by Baudelocque, means air which contains the carbonaceous impurity expelled from the lungs and the skin. Well, what is that impurity but an

increase of carbon corresponding with the decrease of oxygen in the air? It is the carbonic acid which kills, and it is the deficiency of oxygen which causes the function of the lungs to be impaired. Any animal which breathes will become consumptive under the operation of influences which impair respiration. Elsewhere I have shown that cows, when shut up, soon fall a sacrifice to this disease. That monkeys and all caged animals readily acquire it. No matter how abundantly they may be fed, still they become consumptive. *They*, surely, cannot be said to inherit the predisposition. But it may be said that they are deprived of *exercise* as well as pure air, and this operates. Admit that, it chiefly if not entirely, operates in impairing respiration and diminishing the elimination of effete carbon. Dr. Ed. Smith's experiments show this most conclusively.

Just attend to this. "Monsieur Baudelocque admits that air thus "vitiates yields to the blood an insufficient quantity of oxygen, and "in the actual state of science it appears that this may be regarded "as the cause of the evil, yet he attaches little importance to this "explanation. The capital point with him is to demonstrate that "the disease depends upon the respiration of air not sufficiently "renewed. For this and other reasons, which will develop themselves as I proceed, the subject is treated of in this place rather "than under the head of 'a deficiency of oxygenation.'" (Ansell, p. 439.) First of all, is that a correct statement, that M. Baudelocque throws stress upon the deterioration as a producing cause of tuberculous deposit, and the blood receiving an insufficient amount of oxygen?—Yes, deterioration of the air breathed I consider to be a material agent in the production of tuberculous disease.

And deterioration of air is insufficiency of oxygen, is it not?—Not only a deficiency of oxygen, but an increased quantity of carbonic acid and other matters, and other impurities.

Of course, if the oxygen is not present, something else must be, must it not? I mean, nature abhors a vacuum, as we all know, and you must have it filled somehow?—Yes; but that which supplies the place of oxygen may be a positively deleterious impurity, or is often more than a deficiency of oxygen. It often contains impurities which are positively obnoxious.

Is there anything that you can supply to make it as healthy as oxygen?—No.

Anything that supplies its place must be inferior to oxygen?—Yes; but it may also be positively pernicious.

The Lord Chief Justice. If you breathe the expired breath of a great number of individuals, as we do here in the course of the day, that would be positively harmful?

Mr. Coleridge. Take it in that way, which is positively pernicious?

Now, attend to this: "Mons. Baudelocque regards the respiration of an atmosphere not sufficiently renewed as the sole cause of tuberculosis (scrofula). Giving this expression its true interpretation, it means, that the air within the chest, by which the true process of respiration is effected, contains habitually a larger proportion of carbonic acid and nitrogen gases (irrespirable air), and a smaller proportion of oxygen gas (respirable air), than is consistent with the due reaction of the blood." Is that correct?—I think there is a great deal of truth in that.

"Of all the causes suggested as capable of producing a tuberculous state of the blood, I believe that a vitiated atmosphere, in the sense adopted by M. Baudelocque, is one of the most frequent and effectual; but I do not believe it, especially in the restricted sense, to be a sole cause. Rilliett and Barthez ascertained, that a vitiated atmosphere possesses a great influence over the development of internal tubercle in young persons; of 57 children subjected to it, 40 died tuberculous and only 17 non-tuberculous;" and so on. So he goes on through the different parts. Then I may assume that it is essential to health to get rid of carbon in an undue proportion; and that if it remains it is destructive to life?—Injurious to life.

But is the one mode of getting rid of carbon to consume it by oxygen, and oxygenate it?

The Lord Chief Justice. That is, the only mode?

Mr. Coleridge. I said *the one*, my Lord.

Is that the only mode known of getting rid of carbon?—Through the lungs.

To oxygenate it?—Yes; it is scarcely a proper expression, "getting rid of carbon;" oxygen has more power than merely getting rid of carbon.

That was not my question. I ask you whether the presence of carbon, unoxxygenated, is prejudicial, and you say yes; and I ask, is not the one means of getting rid of it to bring it into contact with oxygen?—Yes, if it is by artificial means.

The Lord Chief Justice. You are speaking of Dr. Hunter's process?

Mr. Coleridge. Yes.

That is the ordinary mode, is it not, to oxygenate it?—Yes; but, remember, it does not oxygenate it directly. Oxygen absorbed into the blood, does not combine with the carbon in the lungs.

I know that very well—it is absorbed into the blood and passes through the whole system; but it renders all the functions of the body active by its process. Is not the oxygenation of the blood

that which turns to red from blue, and arterial from venous?—Certainly.

And if you cannot get enough oxygen?—Then it remains black.

If you cannot get enough, you cannot oxygenate the blood?—Quite true.

The Lord Chief Justice. Therefore it remains with the worn-out tissue which ought to be got rid of?—Yes.

Mr. Coleridge. You say this passage is also contrary to fact: “Now, “since death follows, in from one to two minutes, from simply shutting off the air; you can readily understand that even the slightest “diminution in its quantity or purity must be followed by great “injury to the health.” “The blood always becomes carbonaceous “when the quantity of air is diminished, and the quantity of air is “always diminished when the air-tubes are thickened by catarrhal “inflammation, or clogged by sticky mucus. Consumption is only “the consequence of this carbonaceous impurity of the blood. “Tubercles themselves are only deposits of carbon in the tissue of “the lungs, and the ulceration and destruction of this important “organ result directly from their presence.” Now, what part of that do you say is contrary to the fact?—“Consumption is only the consequence of this carbonaceous impurity of the blood.”

What do you say it is the consequence of? First of all, is it the consequence of that amongst other things?—That may contribute to degrade the condition of the blood and bring it to such a state as to make it capable of depositing tubercle; but imperfect respiration is a very imperfect cause.

Carbonaceous impurity—you say that may contribute?—Yes, that and other things.

May it mainly contribute?—I would hardly go so far as to say mainly.

What do you think mainly contributes—a degradation of the nutritive materials of the blood?

The Lord Chief Justice. What do you mean by a degradation?—It is rendered inferior in its quality, so that it is no longer capable of making good flesh.

Mr. Coleridge. Is that from the presence of too much carbon and too little oxygen?—That is very questionable—so far questionable that it is the opinion of many pathologists that excess of oxygen tends to the development of tubercles.

Here the witness tried to wriggle out of giving an answer to the question by putting forth an opinion which he himself did not believe, and which he knew was distinctly the opposite of the views enunciated in his own book. This he is forced to half admit in his reply to the next question. It will deteriorate the health—it will deteriorate the blood—it will do mischief; but he

will not admit in plain words that it will develop scrofula and Consumption, though he admits this distinctly in the passage from his book I have already quoted.

That may be; you may have too much of a good thing, you know; but according to your notion, may it result from there being too much carbon and too little oxygen in the blood?—No, I think that is a very small matter. That would deteriorate the health, and therefore that deterioration of the standard of health may promote the production of consumptive disease; but not by any direct mode leading to the deposit of tubercle in the lungs.

Then, do I understand you that it may be, if not a main, at all events, an important cause, and may contribute indirectly though not directly?—It may contribute indirectly.

That may be admitted as deteriorating the general health?—When you say may be admitted, I know nothing about it.

Mr. Karlake. It is only a form of expression.

Mr. Coleridge. Well, that may be admitted—how much of this is contrary to fact?—The “only.”

“Only the consequence of carbonaceous impurity of the blood”—the word “only?”—Yes.

The tubercles being only deposits of carbon in the tissue of the lungs; that you would say is incorrect?—Yes, utterly incorrect.

Do you think that, before the deposition of tubercle in the lungs, there is always a diminution of the vital capacity of the lungs?—You mean by vital capacity of the lungs the area of the lungs?

I cannot tell you what I mean by it; but I will read you the passage.

The Lord Chief Justice. It is one of those scientific terms one meets with sometimes, which may mean a vast variety of things.

Mr. Coleridge. I was going to ask Dr. Williams what he meant by it. Here is Dr. Edward Smith; he knows something about it; he is of the Brompton Hospital, I believe?—He was.

He knew something about it then?—Yes.

“We have tested this question with every care, and in many persons, and believe the proposition to be true in every period of the early stage of phthisis. No one will deny that the vital capacity is lessened where there is a material impediment to inspiration such as occurs with deposited tubercle, but many may question it in the absence of this deposition, and affirm that if it exist there must also be the tubercle. We shall discuss this part of the subject in a future chapter, but here we affirm that there is a considerable diminution in the vital capacity, when there is no evidence whatever of the existence of any deposit.”—Dr. Edward Smith, p. 107. Well, I think I could supply the explanation of that term “vital capacity.” It was a term introduced by the late Dr. Hutchinson, the inventor of the Spirometer. That was invented for the purpose of

testing how much air the lungs could take in for the purpose of breathing, and that capacity which the lungs admitted of, he called the vital capacity; therefore it is tantamount to the term active volume of the lungs; and Dr. Edward Smith, in that passage, implies that *before* the existence of tubercle, there is a diminution in that power of expansion of the lungs.

Do you agree with it?—It is very probable. I cannot tell. It is a thing he has wrought upon and has satisfied himself about. I am uncertain; but this I distinctly say, that I consider, among the predispositions to consumption, certain conformations of the chest—a narrow ill-formed chest, which does not admit of the full free expansion of the lungs; but then that interferes, not only with the respiration, but with the circulation and other functions.

There cannot be imperfect respiration without imperfect removal of the carbonaceous impurity. What, then, becomes of it? If it is not expelled *by the lungs*, then it must remain in the system, or be got rid of by the skin, or some other outlet of the body. Imperfect respiration generally results from “the habitual want of pure air.” This, Dr. Williams tells us (*Principles of Medicine*, p. 53), “*exerts an unfavourable influence on the state of the blood, and on the functions of circulation and nutrition;*” and this unfavourable influence, he adds, produces such changes as “degenerate into scrofulous or tuberculous matter.” Here, then, we have scrofula and Consumption set up through imperfect respiration, on Dr. Williams’s own showing. The blood is brought into a “deteriorated condition” by the want of pure air. It results from the “*respiratory changes*”—i.e., introduction of oxygen and expulsion of carbonic acid—not taking place as perfectly as they ought. This he also admits; for he says, “*When the respiratory changes of the blood are reduced by disease within a narrow sphere, it becomes an object not to increase the hydro-carbon of the blood by the use of food with much fat*” (p. 188). Here he speaks of respiratory changes “reduced by disease.” Now, if we turn to his work on diseases of the chest, we learn that the very diseases he means are those which I have laid down as rendering the blood carbonaceous—“severe colds” and “inflammation, whether pulmonary, pleuritic, or bronchial, imperfectly treated and only “partially subdued.” These, he tells us, pass “into a chronic

“form, and either immediately develop tubercles in the lungs, “or by lowering the vital powers, generally lead to their formation from perverted nutrition, or from the irritation of any “fresh exciting cause.”

There is not a particle of difference on this point between Dr. Williams's real opinions and my own. My book explains all these influences even more plainly than his; and yet I wrote for the public and he for the profession.

Now I go on to the next matter, page 10, in Dr. Hunter's book, which you say is a gross exaggeration of the fact. After speaking about catarrh and granular sore throat, he finishes up by saying, “The public do not require to be told that catarrh is the most “common source of deafness, from causing the closure of the “Eustachian tubes; and that frequently extends itself down the gullet “to the stomach, causing the most inveterate form of dyspepsia. “Chronic irritations of the mucous membrane always extend themselves downward. They are mild and easily cured in the commencement, but become grave and dangerous when neglected. I regard “catarrh as the great feeder of pulmonary irritation; and by no other “means can we so effectually guard the lungs from disease as by “promptly removing the catarrhal affection.” You said, when my lord put it to you, if it was meant to be chronic and neglected catarrhs, you should say they did not lead to consumption?—No.

Not chronic and neglected catarrhs?—No.

What do they lead to?—They may be cured.

I am speaking of chronic and neglected catarrhs?—A person has a chronic and neglected catarrh in the winter, and may get rid of it in the summer.

But he may not?—He may not, and he may have it last for a long time—the whole of his life, and have what is called an old man's catarrh.

When you gave your evidence on this subject, did you understand Dr. Hunter to be speaking of ordinary cold, or of a neglected cold, going on from bad to worse?—A neglected cold.

You still think it a gross exaggeration?—I think it an exaggeration.

He says they are mild and easily cured in the commencement, but become grave and dangerous when neglected?—Yes, that is what I say; neglected colds have not that result.

They are not grave and dangerous?—Not in the proportion here represented.

The Lord Chief Justice. Represented here as the cause of consumption?

Mr. Coleridge, “They are mild and easily cured in the commence-

"ment, but become grave and dangerous when neglected?"—I say that is an exaggeration ; for cold is common enough ; and we have not always the means of attending to our colds, but happily they do not always prove grave or dangerous.

Do you mean to say that a chronic or neglected cold is not a dangerous thing?—Not by itself. If there is consumptive tendency, or other causes attending, it may be.

Would not you say that prudence was to remove the cold?—I say the tendency here does not lead to consumption ; it may have dangerous effects of other kinds, but not that it *necessarily* leads to consumption.

These are all matters of *opinion*, and not of fact. At first my remarks were a "gross" exaggeration, then he comes down to "an" exaggeration. And yet he admits in his book that most of his patients attribute their disease to some former attack of cold which they never could get rid of. I attribute to such attacks the actual setting up of the disease. He does the same, but then he assumes that the health of the system must previously have become deteriorated. That may or may not be true. No patient is competent to judge. He goes to the physician to find out whether his cold be simple or serious. Dr. Williams thinks he ought to wait and trust to fate rather than go to Dr. Hunter. If the patient would go to Dr. Williams he would not certainly insult him for desiring to have his chest examined. But the whole evidence is evasive, and based upon *recent colds*, whereas my book is only on *chronic diseases*. It does not profess even to describe the symptoms of acute diseases of the throat and lungs.

As I limited my remarks on *catarrh* to that chronic form of it which follows frequent attacks of colds, and is characterised by altered mucus, or muco-purulent matter in the nostrils—his observations, so far as they apply to *recent colds*, have nothing to do with the case. I repeat here, that only a limited number of cases assume the form described by me as *chronic catarrh* ; but these, where they occur, afford presumptive evidence of scrofula, and a strong tendency to Consumption.

The Lord Chief Justice. There is another passage, "Chronic irritations of the mucous membrane always extend themselves downward;" is that true?—Well, that is not at all certain. Sometimes they go upwards and sometimes downwards.

The Lord Chief Justice. Some persons may begin with a cold in the chest, and end with a cold in the head ; and others the reverse ?—The word “Catarrh” means flowing downwards—the derivation of the word applies to a tendency in that way.

The Lord Chief Justice. It may begin at the nose, and go down to the throat ?

Mr. Coleridge. Yes, that is what he says.

I think you say this is a gross exaggeration, also, that consumption causes one death in four ?—It exceeds the statement in the statistical returns. I think it is somewhere about one in eight and a half.

That is, including all deaths ?—Yes.

Attend to this : “ Continued to no age, sex, or condition of life, these “ pulmonary diseases destroy a larger proportion of mankind in temperate climates than all other chronic diseases taken together. In “ this country, and over the whole temperate region of Europe and “ America, tuberculous disease of the lungs causes probably a fifth “ part of the whole mortality.” That is, over the whole of Europe and America. “ And in some districts, and even in whole countries, “ the proportion is much larger. It has been calculated by the late “ Dr. Young, Dr. Woolcombe, and others, from the best data which “ the bills of mortality afford, that in Great Britain and Ireland consumption causes one-fourth part of the deaths that occur from “ disease.”—(Sir James Clark, p. 8.)

You think that is a gross exaggeration ?—That is an impossible statement with regard to the present time. Dr. Young lived some fifty years ago, and the bills of mortality were very imperfectly kept at that time ; and a great many deaths were put down to consumption that would not be put down so now.

You mean that some things are now put down under the separate heads of bronchitis, and so on, that would formerly have ranked as consumption ?—Yes.

Do you mean to say that, including cases of bronchitis and other heads of disease more separate, which were formerly classed under consumption—with regard to those that the returns show there is, on the whole, a diminution ? If you have not looked at them, I will not press you ; but if you have, I must do so ?—I will not be particular as to the figures.

If you say you have not looked at them, I will pass on ?—The figures I am not sure of.

The Lord Chief Justice. This means those that result from pulmonary disease.

Mr. Karlake. He says, expressly, “ I speak of the nose, throat, “ and bronchial tubes.”

The Lord Chief Justice. The statement of one-fourth or one-fifth of the proportion of those who die of consumption, has reference to tubercular disease alone.

Mr. Coleridge. I am not sure : “ The weekly bills of mortality

"point us to the startling fact, that of those who have passed the age of puberty fully one in every four persons we meet in the great thoroughfares of business and pleasure is under the ban of this terrible disease. The deep interest and great importance of everything which promises to mitigate the ravages of this scourge cannot, therefore, be over-estimated."

The Lord Chief Justice. "In my last letter I concluded my observations on the diseases of the nose, throat, and bronchial tubes." And then he says, "I now come to speak of pulmonary consumption, that dread malady, in which these affections, when neglected, invariably end."

Mr. Karslake. That is made clear a little further down: "Fully one in every four persons we meet in the great thoroughfares of business and pleasure is under the ban of this terrible disease."

Mr. Coleridge. But bronchitis was heretofore regarded as one with consumption; or at least, was classed under the same head?—That was in times when people could not distinguish between the one disease and the other.

The Lord Chief Justice. It was only applied to chronic bronchitis?—No.

Acute bronchitis? Yes; cases of acute bronchitis, with purulent expectoration; and they were put down as cases of consumption.

Mr. Coleridge. I should like to have the fact. Do you pledge yourself at all, or have you an opinion? If not, I will pass over it. Do you mean to pledge yourself that deaths from diseases of the lungs, whether you call them consumption, or bronchitis, or what not, and that class of disease, are less than one in four now?—I DISTINCTLY PLEDGE MYSELF TO THAT: that they are less, according to the most recent estimate.

I am glad to have this definite statement; for it enables me to prove, beyond dispute, that both he and the other "scientific witnesses" are not so well informed, in regard to the death-tables of this country, as they should be. If I have not understated the facts, I am very much mistaken.

The classification of diseases has undergone, as Dr. Williams admits, very great changes. Formerly, the cases of Bronchitis were set down under the head of Consumption; and, indeed, the majority of them so closely resemble the latter disease in general symptoms, that they can only be distinguished by the stethoscope. Under the old classification, acute Bronchitis was chiefly set down as Bronchitis, and chronic Bronchitis and Consumption united together in one sum.

If we take the deaths for a period of five years just before the

introduction of Cod-liver-oil, we find that in a population of 15,720,391, the average number of deaths from all causes was 364,890; and of these, the deaths from diseases of the respiratory organs were 91,894. In other words, $21\frac{1}{2}$ persons in every 1,000 died from all causes, and about $5\frac{3}{4}$ of these died of diseases of the lungs. This is a little more than *a fourth*. Now, if we compare this with five years from 1860 to 1864, we find that in a population (averaged for the five years as in the former case) of 20,316,987, the annual average of deaths from all causes was 452,754, and from diseases of the respiratory organs 125,061. Hence, $22\frac{1}{2}$ in each 1,000 persons died from all causes; and of these $6\frac{1}{4}$ deaths were from diseases of the lungs. This shows a steady increase in the mortality. The increase in population was only $29\frac{1}{4}$ per cent., while the increase in mortality from disease was $30\frac{1}{2}$ per cent., and the increase in the deaths from diseases of the lungs $36\frac{1}{10}$ per cent. Here, then, we find that the deaths from diseases of the lungs cause considerably more than a fourth part of all the mortality, even including accidents and infantile maladies. If these are signs of medical progress, the medical men of England are entitled to all the honour!

Now, if we take the deaths from Bronchitis for the five years ending 1842, we find that the aggregate mortality of those years from this malady was only 10,677—a correction for the difference of population should have given us 13,776 for the five years ending 1864. Now what do we find was actually the registered mortality of this latter period? Why the astounding total of 166,953, or an increase of more than *fourteen hundred per cent. above the corrected average!* This is what Dr. Williams spoke of as a “separation of Bronchitis from Consumption by “reason of improved diagnosis.”

This is how a small decrease is made to appear in the deaths from Consumption. Instead of “doctoring the patients,” they must have “*doctored the statistics!*” When his Lordship inquired whether there would be a diminution, if the cases registered under the name of Bronchitis were “*put back;*” and the two added together, as was the case before the introduction of Cod-

liver-oil, Dr. Williams said there would be found to be a *decrease* still. Let us now see if this be true.

If we add the deaths from Bronchitis and Consumption together, we find the following result:—

For the five years ending 1842, the deaths from Consumption and Bronchitis were 308,467. Now a correction for difference of population would have given us for the second period of five years (ending 1864), 398,660, instead of which there were 424,888, or an increase of 26,228 over what it should have been, had the same rate of mortality continued under the blessings of "*fish oil*," that existed before its introduction.

We now come to my statement, that of those who have passed the age of puberty, *one-fourth* are under the ban of this disease, and destined to fall a sacrifice to it, if no improvement be made in the treatment commonly pursued. To the correctness of this statement I still adhere, with one important modification. I made the statement honestly, perhaps without such close calculation as I certainly would have instituted, had I dreamed that there were to be found six medical men in London willing and capable of disputing it upon oath. Now that I have looked *more* carefully into the matter, I find that I was slightly in error. *The proportion stated by me is too low*, it should have been *more* than one-fourth. Puberty is that period of life ranging between 10 and 15 years. Let us, then, fix its commencement at the first-named age, and allow my statement to cover all above that age up to 100 years.

I have before me the statistical report of the Registrar-General for 1864. In the Supplement we find the deaths ranged according to the ages. There died in the ten years, from 1851 to 1861—

All ages	4,579,500
Below the age of 10 years . . .	1,994,143

Deaths above 10 years 2,585,357

Number of Deaths	2,585,357	$= 5.079$
Registered as Consumption . . .	508,923	

But Dr. Williams admitted that Bronchitis had of late years been separated from Consumption. In this admission, however, he could only have referred to cases of *chronic* Bronchitis, which so closely resembles Consumption as to be often mistaken for it. Leaving, therefore, the full rate of mortality from Bronchitis which existed prior to the introduction of Cod-liver oil, and merely restoring the surplus to Consumption, we find the following result :—

$$\frac{2,585,357}{656,539} = 3.93$$

But this does not reveal all the peril to health which these diseases imply. Laryngitis, when it terminates fatally, is, in ninety-nine cases out of one hundred, Consumption, complicated with Laryngitis. This Dr. Williams admits; for in his Lectures, published in the *Medical Times*, he says, “I have never met with a case of chronic Laryngitis terminating fatally, unaccompanied by disease of the lungs.” Then there are several thousand deaths each year, arising from causes distinct from inflammation; these are registered under the head of “*Lung disease*”—almost all of these should appear under the head of Consumption. These two diseases add 42,165, and increase the peril as follows :—

$$\frac{2,585,357}{698,704} = 3.70$$

Still we have left all the other forms of tuberculous disease, which arise from the same condition of the system, and are generally complicated by *tubercles* in the lungs.

Scrofula

Tabes Mesenterica

Hydrocephalus

A great number of persons dying of Consumption are registered under the head of the prominent condition which seems to terminate their sufferings. *Diarrhœa* is one of these, and, indeed, is very frequently the immediate cause of death. Then, again, others are set down as dying of *Debility*, or *Marasmus*. We cannot hope to get nearer to the truth; but nothing is more

certain than that considerably more than one in four of those who have passed the age of ten years will die of Consumption and its cognates, unless something more can be done for the disease than has hitherto been accomplished. Fools may prefer to remain in ignorance; but wise men naturally desire to know the naked truth.

The Lord Chief Justice. This class of disease?—Consumptive disease only.

Mr. Coleridge. You do not attend to the question?—Consumption only.

You are not attending to my question; you are looking at the consequence. I am asking you a question of fact. Do you mean to pledge yourself to this; that, supposing the diseases were now classed as they were when Sir James Clark wrote his book, that statement would not be true?—No; I think it would be scarcely accurate. But what date do you allude to?

The Lord Chief Justice. I think I can make you understand it. Supposing the diseases which you say were formerly set down to the score of consumption, though they were not consumption, were included in the present day under the head of consumption, would not then the statement contained there, that one in four of the deaths is due to consumption, be correct?—No; I think it would be an exaggeration, even if it included those cases.

Mr. Coleridge. What proportion would it bear, should you say, altogether, including those cases?—Including those cases, I can scarcely form an estimate. I know enough of the approximation to be able to say that the mortality from tuberculous disease is somewhere *about eight per cent.* I am not sure of the unit or decimal; but it would be an exaggeration to say that, in temperate climates, it is more than *eight per cent.*

Has bronchitis largely increased?—No; not that I am aware of.

Not the deaths from bronchitis?—Well, I believe it may be said that the register of deaths shows more, for the reason I have mentioned.

The Lord Chief Justice. Taking it from one category and putting it in the other?—Yes.

It will excite no surprise in the reader's mind that Dr. Williams was so sadly out in his statistics when he reflects that he did not even know the composition of the blood, and had forgotten his own written opinions on many points. But here is something which will surprise everybody, though, after all, why should it? Is it not natural that a physician who writes one thing in his book, and testifies directly the opposite in Court,

should be unable to distinguish the difference between *one* in *eight* and *eight per cent*! I wonder, now that his attention is drawn to the matter, whether he will be able to solve the difficult problem! He did not know that Bronchitis had *increased*, when, according to the registered deaths, it had actually risen from a little over *two* thousand deaths in a year to *forty thousand*! But then, says he, these have only been taken from the Consumption List. Well, add them together, and you still find that, after making the correction for difference of population, there is 26,228 deaths to be charged against the two diseases in the *five* years ending 1864 *above the rate of mortality of that happy period which preceded the fish-oil mania*! And, what is worse still, all the other diseases of the lungs have also *increased*!

Mr. Coleridge. Now, upon these symptoms of consumption, there is a whole chapter, from page 40 to page 45, which is the first letter on the symptoms of consumption. Do I understand you to say that; looking at that whole letter, which I assume you to have read?—I have read the whole letter.

Have you read it carefully?—Carefully.

Then do I understand you to say that the whole letter gives an untrue account? That where those symptoms are present together, and present for a long time in a chronic condition, a man is not suffering from consumption, or probably suffering from consumption?

Mr. Karslake. You are mistaking the passages. If you take the first, it is a cold.—A great deal of this seems to be a detailed description of the symptoms of disease which may be consumptive or may not be consumptive.

Mr. Coleridge. I am quite aware of that. But what I ask is this, whether, if the symptoms set out in this letter co-exist, and exist for a considerable time, you would not, as a medical man, consider the person who had those symptoms, and had had them for a considerable time, had better look after himself, at all events?—I should say there is an awful delineation of symptoms. The whole category of symptoms is such, that I should be very sorry that any of my friends should suffer from them.

You would think they had better look to their lungs, if they did?—Yes; that is with regard to the whole description. That does not apply to the isolated passages.

Here Dr. Williams pays me an unintentional compliment. He thinks my description so *full* that he should be sorry if his friends suffered from them. So would I, and equally if he himself were so unfortunate; but then, if they *had* the symptoms

delineated, would they not be in great danger? Would it not be imperatively necessary for them to be treated? If, on the other hand, they *had not* the symptoms described by me, then they would be made happy by the conviction that they were in no danger. Then there is another good gained by instructing non-professional people in the early symptoms of disease. They are less likely to neglect themselves; they apply to the physician before the disease is too firmly established, and they are less likely to be misled in regard to their real situation by his words while under treatment.

Did you suppose, in giving your answers, that he was speaking of something that occurred once or twice, or that he was speaking of symptoms that were co-existent and that became chronic?—In which passage?

That you say is a gross exaggeration and not founded in fact.—Applied to which question?

In such an instance as this, at page 40: “Instead of the promised return to health, the poor invalid finds all his symptoms steadily growing worse, and becomes importunate for relief,” and so forth; and “To quiet his fears he is now advised to visit the country, and to wait for the spring. And then he finds that is no good;” and then: “One of the earliest signs of consumption is cough. For a considerable time this is so slight as to be entirely overlooked by the patient, and may scarcely be noticed by his nearest relatives, it being in reality little more than a dry hack. It is most commonly observed in the morning on first getting out of bed; but it may also occur during the day, after meals, and after walking and conversing. Some weeks or months later, varying with the progress of the disease, the morning cough is attended by the expectoration of a clear fluid, like saliva, and generally frothy. After a time little points of a pale yellow matter make their appearance in the frothy mucus; and as the disease advances, this increases, until it almost takes the place of the clear mucus. There is no uniformity in the amount expectorated in this disease. Occasionally the quantity is very small, even where there is extensive disease of the lungs; while, on the other hand, it may be profuse, steadily increasing from the commencement, until it reaches, in the last stage, half-a-pint or more in twenty-four hours. In many advanced cases the sputa looks like little balls of cotton or wool, and in others of a greenish yellow colour. Now, when a dry hacking cough steals upon a person in apparent health, and without the occurrence of a cold, it should always excite apprehension;” and then he goes on, “If, therefore, you have a hacking cough and slight shortness of breath on exertion, accompanied by an increase in the frequency of

"the pulse of 10 or 15 beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal." Do you consider that a gross exaggeration?—Yes, that answer applied to that description which you have just now read.

You think if a person had a long hacking cough going on for a length of time——?—There is nothing stated about going on for any length of time.

The hacking cough "some weeks or months later, varying with the progress of the disease," and so on—you say there is nothing about its going on—"shortness of breath in the early stage is very slight, for the amount of obstruction in the lungs is small; but if it keeps pace with the progress of the disease," and so on; and then you think it was fair to separate the end of the chapter from what had gone before, and treat it as a separate thing?

Here we have a desperate attempt to isolate a little passage, and to torture it into some meaning not intended. I will settle the dispute by saying that it is identically the same in substance, and almost in words, with the following:—

Sir THOMAS WATSON says—

"Now when such a cough steals upon a person gradually, and when no reason can be assigned for its occurring" [*i.e.*, the patient being in apparent health], "that circumstance alone is enough to excite suspicion as to its true nature and cause."—(Watson's "Lectures," p. 198.)

The Lord Chief Justice. What he says is, "When a dry hacking cough steals upon a person in apparent health, and without the occurrence of a cold, it should always excite apprehension, and lead to an immediate examination of the lungs." That comes after, "In many advanced cases the sputa looks like little balls of cotton or wool, and in others is a greyish yellow colour."

Mr. Coleridge. That, again, is stating a single passage; for he goes on to say, that may possibly be unimportant—the dry cough does not always end in consumption.

The Lord Chief Justice. That paragraph begins thus: "One of the earliest signs of consumption is cough. For a considerable time this is so slight as to be entirely overlooked by the patient, and may scarcely be noticed by his nearest relatives; it being, in reality, little more than an occasional dry hack;" and then he goes on to say how it may increase; and then he alludes to the expectoration of a clear fluid, like saliva sometimes, and generally frothy; and after a time little points of pale yellow or greyish matter make their appearance in the frothy mucus; and then he says, after that has come to pass, "Now, when a dry hacking cough steals upon a person in apparent health, and without the occurrence of a cold, it should always excite apprehension and lead to an examination of the lungs."

Mr. Coleridge. It is, "have your lungs examined. It may possibly prove unimportant, for dry cough does not always end in consumption; but it is suspicious, and no man who values health will disregard its warning." Then he finishes the whole with, "If, therefore, you have a hacking cough and slight shortness of breath on exertion, accompanied by an increase in the frequency of the pulse of 10 or 15 beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal." Now, I think you said the description about the consumption, that is to say, where those symptoms concur and are found to continue, you would expect to find disease of the lungs?—It is rather a strong term to say that I should expect to find; I should *suspect*, and therefore make an investigation; but the passage principally objectionable is this, "*a person supposed to be healthy*;" many of the observations in that page are not connected with the preceding paragraph.

Not connected with the preceding paragraph? "If a person supposed to be healthy is found to breathe more frequently than this when quiet, and not aware that the number of his respirations are being counted, set it down that he has, more or less, shortness of breath." What is the objectionable part of that?—As far as that goes, it is not objectionable.

You point that out as the objectionable passage. What is the objection to it?—I do not know whether that is the passage which winds up "that is consumption."

No, that is not the passage; here it comes forward: "each additional respiration, as a rule, increases the pulse about five beats. Hence, in consumption, the pulse keeps pace with the shortness of breath;" and it winds up in this way: "If, therefore, you have a hacking cough and slight shortness of breath on exertion, accompanied by an increased frequency of the pulse of 10 or 15 beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal?"—I say that is objectionable, starting as it does with a person supposed to be in health.

You connect it with a person supposed to be in health?—A person supposed to be in health, with these two exceptions, shortness of the breath and quickening of the pulse.

Then if a person, supposed to be in health, has a hacking cough and shortness of breath—and then you admit that a page comes before?—Yes.

Then it is objectionable?—Yes, as an idea of consumption.

All this quibbling over a mere description, which is almost verbatim with that given by Sir James Clark, Sir Thomas Watson, and every authority on this disease that has pretended to describe its symptoms. I will now take this last passage

which Dr. Williams objects to in my book. He does not think it correct. What does he say to the following from

Sir THOMAS WATSON'S ?—

"If without any apparent cause a person grows thin and weak" [loses flesh], "and his pulse be quick and his breath *at all* short, these are intimations which seldom prove unfaithful that tubercular disease is at work in the lungs and in the abdomen."—(Watson's "Lectures," p. 204.)

Is your name Charles J. B. Williams ?—Yes.

And you have written a book about diseases of the chest ?—Yes.

Is this true : "Instead of giving a detailed account of the symptoms of phthisis, I shall endeavour to class them according to the pathological stages of the lesions that give rise to them. * * *

"The cough and other symptoms begin very gradually without any very obvious cause, and with as little apparent external reason soon increase to a serious extent, and the consumption runs a rapid career. In a third class of cases, the patients have been out of health, in a debilitated or cachectic state." (Dr. Williams, p. 172.)

That is a good account of a cough ?—Yes.

The best account that can be given ?—Yes.

I may adopt that as a satisfactory account of the matter ? And now I should like to ask you some questions about cod-liver oil. You think, if I understand you, that cod-liver oil applies itself locally through the stomach ?—That is to say, taken into the stomach, it is absorbed into the blood, and is diffused through every part of the body.

As oil ?—As oil.

Free oil ?—Oil as globules, not free oil as a mass, floating as oil ; but it is subdivided, in the chyle, into what is called the molecular base, which consists of minute oil-globules ; those are taken into the blood, and that blood into the tissues of the body, and go to the construction of all those tissues.

Is that different from any other effete matter ?—No ; all oils have the same kind of action.

All animal fat, I suppose ?—All animal fat in the same way.

Mutton fat, for instance ?—Yes, mutton fat may ; but being more solid, perhaps not quite so easily.

That is what you mean ; do not let me mislead you ?—Yes, quite ; they are all alike ; oil or fatty matter.

Dr. Smith, I think, when he was at the Consumption Hospital, knew something about it, did he not ?—I presume he did ; he is a very highly respected individual.

"It is also true," says he, "that sedatives and narcotics, when taken in pills, or other form, so that they cannot exert a local action as they pass over the sensitive surfaces, yet lessen the sensibility through the general system ; but we submit that this is a very

" circuitous mode of action, and very liable to disturb other parts which do not need the sedative influence. It is certainly better to administer the remedy in such a form that it may act directly upon the part affected, and be administered in small quantities and frequently. We venture also to affirm that cod-liver oil, when it acts beneficially, often does so by the local action upon the throat, producing the soothing effect and relieving the cough, to which so many patients ascribe its utility." (Dr. Edward Smith, p. 320).
—Well, that is a matter of opinion; I differ from it.

You do not agree with that?—I do not agree with that.

But, however, he is a decent man?—Quite so.

Not a scoundrel or impostor?—Oh dear no, he is a man who stands high in the profession.

" Life," says Dr. MacCormack, " in a pure unbreathed atmosphere, with its respiration at all times and places, averts consumption irreversibly. This is more than can be said, were it in a single instance, of the seventy or eighty tons of fish oil yearly consumed in England, or the 600 gallons of the same ineffective substance annually, it has been said, employed in the Brompton Hospital." That is coming home. " Nay, all the cod fish that swim in the mighty ocean, were they converted into oil, could not, would not, relieve or avert, were it but in a single instance, consumptive tubercular decay."—That is the opinion of Dr. MacCormack, *valeat quantum*.

And that is the opinion of Charles J. B. Williams, *valeat quantum*?
—Just so.

Is Dr. Swett an author known to you?—I cannot say I am known to him.

The Professor in the New York Hospital?—Yes.

Are you not acquainted with him?—I am not much acquainted with American medical literature.

See whether you agree with this: " In the first place I willingly admit that no remedy has as yet been discovered which appears to exert any specific influence upon tubercles, either in preventing their development or in promoting their cure. Antimony, digitalis, iodine, have all had their day of imaginary success, and all been forgotten. Cod-liver oil, the present popular remedy, is destined to experience the same fate. It has not, in my opinion, any specific influence in phthisis. It has not, in my experience, formed any wonderful cures. I do not, however, mean to deny its usefulness in this disease. It certainly appears to diminish the emaciation, to improve the appetite. It is good nourishment, nothing more; and I think it very probable that other kinds of oil, equally well prepared, may exert the same beneficial influence." (Dr. Swett, p. 309). Do you agree to that?—No, I do not.

That you say is merely the opinion of Dr. Swett, *valeat quantum*?
—Just so.

Mr. Karstake. Let me look at that passage.

Mr. Coleridge. Now we come to the practice of inhalation. Do I understand you to say, that inhalation, as a mode of treating the disease, not merely palliative and giving relief in particular instances, and at particular times, but as a mode of treating the disease for the purpose of curing it, has been known and practised in your profession for thirty years?—Yes.

What great physician, including yourself—I do not mean it satirically—has ever pretended to use it as a treatment of disease?—Within my recollection and knowledge, Sir Charles Scudamore wrote a book on inhalation, in which he vaunted its powers very highly. That was, I think, about thirty-six years ago—in 1830.

Did he make it a regular part of the treatment of the disease?—Yes.

You say it had no great success?—Well, he published a great number of cases of supposed cure by it.

Of supposed cure, you say?—Yes; that is, of considered cure.

Dr. Williams has kept his knowledge, and experience, and prescriptions a *secret*, if he ever really employed inhalations in his practice, since he has not given us any account of them in his published works. He seems as ready to discredit the dead as the living. Even Sir Charles Scudamore's cases are only "*cases of supposed cure.*" Had Dr. Williams treated them they would have been *real*!

They were not satisfactory—how?—They were doubtful—I would not pass a judgment upon them—I had not an opportunity of seeing them.

But it died out then?—Not entirely; the iodine inhalation, and conium inhalation, and other forms of inhalation which he recommended, are still in use.

Iodine is an absorbent, is it not?—Iodine has an alterative effect on the bronchial tubes; it was given under the idea of producing the absorption of tubercles; but it certainly has a modifying influence on the mucous membrane over which it travels. (See foot-note, page 138 of this book.)

But it is generally considered an absorbent, is it not?—You may give it that term.

I do not know what it is, and I ask you whether that term expresses it?—Well, it is supposed to absorb the tissue or tubercle.

The Lord Chief Justice. Are not there certain vessels in medical language called the absorbents?—There are; but we have no means of acting upon them. It is a common phrase in use for those drugs which excite the absorbents, but it is a mere *façon de parler*.

It is used for that purpose?—For the purpose of promoting and

exciting absorption in some way or other ; we know of no physiological action by which the lymphatic or the absorbent vessels can be stimulated, there is no physiological explanation of such an action.

Mr. Coleridge. Hemlock is a narcotic ?—Yes.

And the only difference in giving into the stomach or by inhalation, is how the constitution may be able to bear it ?—Yes.

There is no difference in principle, which way you give it ?—No, not materially.

Is there any considerable physician, now living, who treats his patients by inhalation, chiefly, as a main part of the treatment ?—That is a strong word, “chiefly.” As I said, with regard to myself, it is an aid, but a subordinate aid, like the smoking of stramonium, and that we have known a long time.

But you do not mean there has been anything like a systematised process of inhalation ?—Well, so far as systematised inhalation goes, I suppose there have been fifty kinds of inhalation invented by different people for this purpose.

If so, not *one* of the *fifty* has been detailed by Dr. Williams. He has kept his knowledge of them, like his own experience, secret. Nobody, not even his patients, many of whom have come under my care, ever heard of Dr. Williams’s *now assumed* knowledge or experience of inhalation.

What purpose ?—The purpose of inhalation.

For inhaling what ?—Various things—hot water impregnated with all sorts of things.

Do you mean as a mode of applying to the system remedies in those cases, which have heretofore been sent through the stomach ?—Yes, many of that same character.

Which ?—Hemlock, creosote, stramonium, opium, asafoetida, camphor, and a great number of others. I do not believe there are any drugs in common use that are used through the stomach, which have not also been tried in the way of inhalation.

That is likely enough, which have also been tried ; but are you aware of any person who has attempted what may be called to rehabilitate the lungs by supplying them with oxygen in the way suggested by Dr. Hunter ?—I do not know about the way suggested by Dr. Hunter.

I will ask you about that in a moment ; but are you aware of anybody who does it in that way ?—I am aware that inhalation of oxygen has been practised in the London hospitals.

To any extent ?—To a considerable extent.

How long ago ?—Within the last ten or fifteen years.

Dr. Williams says that treatment by oxygen has been practised in London to a great extent. If this be true, how is

it that none of those who had so practised were called as witnesses? It certainly is true that many medical men have thought most highly of it; but, such was the hostility of men like Dr. Williams, that these were prevented from becoming known. The following extracts will show what those who have really investigated the subject think of oxygen:—

Dr. BIRCH says (p. 62)—

“The preceding observations naturally lead to some considerations in relation to the therapeutic employment of oxygen in Consumption.”

At page 63—

“Although Consumption is occasionally developed in constitutions not in the slightest degree apparently predisposed to it, from hereditary idiosyncrasy as from temperament, yet, persons of strumous diathesis are so peculiarly marked out for and include such a large majority of its victims, that it cannot but be closely associated with struma and scrofula. As I have spoken confidently of the judicious medicinal use of oxygen, as the great basis of successful treatment in all cases characterised by this diathesis, so I can with equal fearlessness assert that Consumption, except in the last stages, is quite amenable to its influence, and claims its best rendered services.”

So completely is the great respiratory law of nature overlooked in the ordinary management of Consumption, that I cannot in justice omit naming two or three physicians (*rari nantes in gurgite vasto*) who have specially led the way towards the correct principles of treatment.

At page 71 Dr. BIRCH says—

“To simplify our therapeutic measures, to clearly understand their *modus operandi*, and to base them strictly upon the laws of nature, by assisting the vital dynamics (the *vis medicatrix nature*), in *nature's own way*, are requisites which must be followed by all. The proper employment of oxygen gas in consumption fulfils all these conditions; and I hope, before long, to put forward such a mass of evidence as will convince the most sceptical mind of its curative powers. Be it always understood that scientific adjunct means, so ably recommended by others, must never be superseded when necessary, but that they must be regarded merely as assistants, not as curative.”

Dr. RIADORE says (p. 86)—

“Although the proportion of oxygen in the atmosphere is, doubtless, the best for average health; still its volume must vary with the temperature of the inhaled air, consequently medical men have found that, in certain diseases, an increase of oxygen gas over the natural proportion in which it is found in the

"atmosphere is exceedingly beneficial; because oxygen, in warm close rooms, as I have already pointed out, is continually found to be insufficient to preserve health; whilst carbonic acid, under similar circumstances, is found proportionably too abundant."

Again he says (p. 97)—

"The surgeon, who often has immediate access to the seat of the disease, justly considers it necessary to join constitutional with local treatment. Not, however, I must here observe, that I consider the influence of the inhalations to be merely local; for example, a patient with affection of the lungs, liver, or of the digestive organs, will express that from the oxygen he finds an improvement of the appetite, and a greater elasticity of mind or cheerfulness, with gentle perspiration and regularity of the bowels."

And again at p. 99 he says—

"From very extensive trials for some years past, I am fully satisfied that some cases of Consumption are remediable and often curable by inhalation of some of the gases, that would inevitably prove fatal by depending upon administering medicines by the stomach, according to the usual system. I am well aware that this assertion will astonish those who have made up their mind that Consumption is incurable; and they will, as they generally do, set down the few practitioners who believe it as *quacks*. Nevertheless, the results of the practice and *post-mortem* examinations of some scores, convince me that the disease may both be arrested and cured by the plan of applying gases and vapours to the seat of the disease, in whatever part of the lungs it may be seated. The benign effect of the inhalation of oxygen in restoring the normal functions of the digestive and secreting organs, and thereby, sympathetically, the respiratory organs, will be shown in the following cases."

Within the last fifteen years?—I can go back still further—fifty years ago Dr. Beddoes did it.

The Lord Chief Justice. For diseases of the chest?

Your Lordship alludes to oxygen?—Yes.

Not exactly for that, but for various diseases?—I was saying fifty or sixty years ago Dr. Beddoes used it.

I think it unfortunate that Dr. Williams should have referred to Dr. Beddoes. Unhappily for science and humanity, he was cut off before he and Sir Humphry Davy had completed their arrangements; and after that, Sir Humphry, then Mr. Davy, fell into the popular current, and directed his energies to other branches of chemistry. Hear what Dr. Beddoes says of the medical opposition he experienced (p. x., preface):—

"I doubt whether the stricture was sincere; but it has been objected to me, that I have written for the general reader, and not merely for the members of

“ the profession to which I belong. I might reply that, without influencing many minds, I could not hope, during the longest life, to see any satisfactory progress made in the inquiry which I wished to have set on foot without delay. I might add, that, although I am perfectly careless about the class of substances to which a remedy belongs, these were very sufficient reasons for writing more at large and in a popular manner, when the subject was so entirely new. The principles on which the trial of gases in medicine ought to proceed were perhaps not very accurately known to the senior part of the faculty; they were nearly as well understood out of the profession as in it.”

The failure of Dr. Beddoes and Sir Humphry Davy to establish the Bristol Pneumatic Institution arose from a variety of causes, any one of which would have been sufficient to defeat the enterprise. 1st, Dr. Beddoes, the moving spirit, died suddenly; 2nd, the profession, from interested motives, gave it every possible opposition; 3rd, the *necessity* of residence in the institution prevented them from getting sufficient patients to support it; 4th, the great expense attending the treatment, and the complicated and difficult processes employed in generating the various gases; and 5th, their known inexperience inspired doubt and distrust, which their professional brethren zealously fanned for their own interests.

Mr. Coleridge. We were told yesterday that inhalation was recommended by Galen, and even Hippocrates?—Not oxygen. It was introduced by Sir Humphry Davy and Dr. Beddoes, and there was an institution at Bristol for the purpose of employing it.

That was not the laughing gas?—No. It was at the same time that Sir Humphry Davy introduced the laughing gas, and then laughing gas has been also applied for the same purpose; but the institution tried that practice of inhalation; and after it had been tried for some time, it was found to be not so beneficial as was expected, and it fell into disuse.

Now you say that this inhalation has been a well-known system, and practised by persons of credit and position?—Yes.

And you say you do not know how Dr. Hunter has done it?—No, except from what has been communicated to me about half-an-hour ago.

I think you said it had been impossible to discover it?—From this book.

You say you do not know how he administers it?—No, I am not acquainted with the instrument which Dr. Hunter uses. It has been impossible to discover it,

Do you mean by the book, it has been impossible to discover it ?
—Yes, by any study of this book.

You mean, when you said in this Court that it had been impossible to discover it, that you had only asked some patients ; or do you mean you were speaking of the book ?—I was speaking of the book, and communications made by patients of his whom I have seen on the subject.

I need hardly ask you, I suppose, whether you took the trouble to ask Dr. Hunter about it ?—No.

Did you ever let him know that you would be glad, or that anybody in your profession would be glad, to know about his inhalers ?—
“No; all the information I obtained about it was through the public advertisements.

As far as you know, did the proprietor of the *Pall Mall Gazette* ever undertake to ascertain what it was ?—I do not know.

Did you ever go to Maw and Son, the instrument-makers, to look at it ?—I have seen the advertisements of them, but I never saw them further.

You never thought of inquiring at Maw and Son’s ?—No.

Nor of any one else ?—No, I have inquired of these patients what they had done.

But you have never gone to the instrument-makers’ ?—No.

And your inquiry has been limited to reading the book and asking the patients about it ?—Yes.

Did you read that portion of the book in the 32nd page—“The inhaling instrument, which is made of glass, and holds about a pint of fluid, is half filled with cold, hot, or warm water, according to the nature of the case. The medicines prepared for a dose are then added,” and so on ?—Yes, I read that.

That gave you no information about it ?—Perfectly vague. It tells us it is made of glass, that is all.

That gives you no idea ?—No exact idea.

The Lord Chief Justice. Could oxygen be brought into contact with the lungs, except in the form of gas ?—No.

Mr. Coleridge. Did it ever occur to you that it might be possible, whatever it was, it might be conveyed through the water ?—I know of no mode in which oxygen could be conveyed through the water.

Could other gases or vapours ?—Gases ; not through the water, but blended with the vapour of the water.

Do you mean they cannot be brought through water by the conductor ?—Yes, but the water would not retain them.

But can they be brought through water ?—Gases ?

Yes ?—Certainly.

By inhaling it through the water from the external air. I mean from something outside the water ?—From a gasholder.

The Lord Chief Justice. I suppose it was for the purpose of conveying the gas by itself.

Mr. Coleridge. It may be done in two ways ; you may either breathe the actual vapour——

The Lord Chief Justice. Do you mean the oxygen gas ?

And yet Dr. Williams *tries* to get more oxygen into the blood by putting acids having “*much oxygen in loose combination*” into the stomach. He thinks acid vapours inhaled would not give up oxygen to the blood, although it is a much more natural and direct process.

Mr. Coleridge. The vapour of the water. You may take it in the form of hot steam, hot medicated steam. Of course, there must be air with it, or have it brought through the water by the process of breathing ?—That is a mere mixture of air with water.

But there are such things as pipes ?

Mr. Karlake. But then they have the atmospheric air outside them ?—My inquiry of the patients was, whether in the inhalations, whether a gasholder was used, which could contain oxygen. It is the only mode I know of in which it could be administered ; and the answer has always been that no such instrument has been used as could hold oxygen gas separately.

Do you mean that it could not be evolved from the various materials used together, and get into the water ?—There is a process which has been invented by a German, Dr. Siegel, with the peroxide of hydrogen, or something of that description, but that is quite recent, and has only been introduced within the last few months.

Do I understand you to say that you admit that chloric acid contains both chlorine and oxygen ?—Yes.

And that if they were disengaged, it would produce irritation ?—Yes.

Is chlorine ever given in diseases of the lungs ?—It has been tried in a very diluted form, but abandoned, in consequence of its producing irritation.

Does it keep its place in the pharmacopœia of Brompton Hospital ?—I am not sure whether it is so now, because it has been used by Mr. D—— at the same time that I was there. But I may say, from my own knowledge of the matter, that its irritation is such as to make it rarely available.

But you are aware it has been used ?—Yes, I am aware it has been used.

You think without success ?—Yes.

The Lord Chief Justice. You say the mischief of it, as I understand you, is that it produces irritation ?—Yes, it produces such a quantity of oxygen from the chloric acid as to be available for any purpose ;

there would be simultaneously so much chlorine evolved from the chloric acid, as would equally do harm.

If you have the chlorine, you can dilute it at your pleasure?—Yes.

But if you evolve oxygen from the chloric acid you must take it with the chlorine?—Yes.

Mr. Coleridge. But you could also dilute chloric acid, could not you?—Yes, you may dilute chloric acid at any moment.

You may dilute it to any proportionate extent, may you not?—Yes, but the more you dilute it, the less oxygen the liquid contains.

Should you consider this to be a correct summing up of the state of consumption? It is the conclusion of the historical sketch of consumption and its treatment at the beginning of Dr. Smith's book—"Hence, as a final expression, we may state that whilst we have "selected a class of cases with an improved diagnosis and pathology, "and these improved views are so largely shared by all practitioners "that the treatment is now similar in all parts of the kingdom, we "are not agreed as to the essential nature of the disease, have no unfailing mode of treatment, and the disease is still essentially and "almost as universally a fatal one as it has been in all ages?"—(Dr. Edward Smith, p. 30.)—No, I think that language is too strong.

That was published in 1862 by Dr. Edward Smith?—Yes. Dr. Smith, I may be allowed to observe, is a man who is highly scientific; but, till he came to the Brompton Hospital, he had had very little experience in the treatment of the disease at all.

Was he there then?—He was there some years before.

I see he calls himself in the preface, "Assistant Physician to the "Hospital for Consumption and Diseases of the Chest, at Brompton?"—Yes.

How many years did he come there before 1862?—I should think four or five years before that.

He had four or five years' experience, then, at the Brompton Hospital before 1862?—Yes. Perhaps you will hear the opinions of the other physicians of the Brompton Hospital on that point as well?

When were you first consulted about this case?—I should think twelve months ago—no, not so long as twelve months. I really am not quite sure when it was, but some time during the last winter.

Who consulted you?—The Solicitor for the Defendant.

Had you read the book before?—No.

You had seen the advertisements?—I had seen some of the advertisements.

Was that the first communication you had about it from the Attorney for the Defendant?—Yes, that was the first.

Did he communicate with you first, or you with him?—He with me.

Had you communicated at all with the *Pall Mall Gazette* previously?—No, I had not seen the article.

Then you saw it, I suppose?—It was shown to me at that time.

Then, if I understand you, you had nothing whatever to do with communicating any information whatsoever to the *Pall Mall Gazette* previous to the writing of the article?—Nothing whatever.

Do not suppose for a moment that I suggest you had; but I merely wanted to have the fact out that you had not. Did the cases that you heard yesterday induce you at all to think that there was anything in Dr. Hunter's system of treatment?—I am sorry to say that I could only get to the gallery, and my present defective state of hearing prevented me from understanding what was said.

You gave some evidence about it, and I thought you might have heard?—No.

Re-examined by MR. KARSLAKE.

Whatever may be the views as to the fatality of the disease, or the curability of the disease, as far as you are aware as a scientific man, is it a fact that of late years the remedies have been far more efficacious which you have applied to it?—I think I may fairly answer that the efficacy of the remedies has increased, more particularly that one remedy—cod-liver oil.

The Lord Chief Justice. That is a newly-introduced remedy?—Comparatively new.

Within what period?—Within a period of twenty years. It has been used in isolated cases before that; but its general introduction had not taken place when the oil was used in a simple pure form. The coarse oil had been used for some time before, but it was so offensive that it could scarcely be properly administered.

Cod-liver oil was introduced by Dr. Hughes Bennett in 1841. It certainly was as *pure* twenty years ago as it is to-day. At the end of twenty-five years' trial both in and out of the hospitals, and after everybody has been *saturated* with oil sufficient to *preserve* them if it had any virtue, we discover that the fatality of pulmonary diseases has steadily increased, that the death-rate is higher, and that all this pretence about the decrease of Consumption is either a transparent delusion or a systematic fraud upon the public. Dr. Williams's admission that they have been taking from Consumption and adding to Bronchitis, explains the whole process by which a *positive evil* has been made to appear as a substantial good. Of course I do not mean to say that Dr. Williams knew that this was the case. I think it is more than probable that he was in the most felici-

tous ignorance as to the death-tables of his own country. And this no doubt explains the delusion of his mind in regard to cod-liver oil. By *taking off* so many thousands each year from Consumption and putting them on to Bronchitis, he fancied he was *reducing* the fatality of the former disease, and doing good, when the two united produce a greater fatality now than they did twenty-five years ago !

I see that a work by Dr. John Swett was cited from, which was published in 1853 ; and he there says, "Cod-liver oil, the present "popular remedy, is destined to experience the same fate. It has not, "in my opinion, any specific influence in phthisis. It has not, in my "experience, performed any wonderful cures. I do not, however, "mean to deny its usefulness in this disease. It certainly sometimes "appears to diminish the emaciation, to improve the appetite. It is "good nourishment, nothing more." (Swett, p. 309.) In your judgment, is that justified by experience, or is it still used to a great extent?—I believe it is still used to a greater extent than ever. The matter can be ascertained by the consumption of the article. I only know by my own practice, and I use it fully with as much confidence and satisfaction in the results as I did fifteen or sixteen years ago, and more so.

You are asked about an instrument or implement which is mentioned at page 32. Just turn to that page, and tell me whether it refers to anything else than administering medicines in bronchitis. In fact, it speaks for itself: "The first object of treatment is to "cleanse the air-tubes of the lungs by expectorant inhalations. "Having attained this object, we next seek to allay the irritation, "on which the secretion depends—to soothe and heal the inflamed "surface. The manner of administering medicines by inhalation, in "bronchitis, is as follows:—The inhaling instrument, which is made "of glass, and holds about a pint of liquid, is half filled with cold, "hot, or warm water, according to the nature of the case. The "medicines prepared for a dose are then added, and the patient "directed to inhale gently, but deeply, into the lungs ; being careful "to expand the chest well, without straining or violence. The fluid "being medicated thoroughly, impregnates the air with its properties." I will just ask you to turn from that page to page ix., in Roman numerals, of the preface to the fifth edition, in which the writer of the preface says : "He proposes another mode of administering medicinal agents, by which, taking advantage of the natural "act of respiration, these can be safely and effectively drawn into the "lungs through the natural channels, by proper preparation, suitable "apparatus, and due precautions." You have been asked whether you ever went to Messrs. Maw and Son for the purpose of getting these inhaling instruments. Do you find any indication in the book

at all that these instruments are to be had there, or are sold there?
—No.

One word about inhaling oxygen. What is the new invention that has been discovered, by which oxygen may be evolved and inhaled?
—I am not acquainted with it, but I suspect it may be a peroxide of oxygen.

As to inhalation generally, there has been reference made to Sir Charles Scudamore's work. Does Sir James Clark, whose book was also cited, likewise refer to inhalation?—Well, I do not recollect. I am not sure on that point. I may mention, by the way, that Sir James Clark was very incredulous as to the value of medicine at all.

This is, to say the least, a very *significant* fact. Sir James Clark, Physician in Ordinary to the Queen, and a great authority in medicine, is said by Dr. Williams, while under *oath*, to be "*very incredulous as to the value of medicine at all.*" I know that such was the case some years ago in regard to the ordinary treatment of consumption through the stomach, for he says in his book—

"The total inefficacy of all means hitherto adopted for diminishing the frequency or reducing the mortality of this class of diseases, is of itself sufficient incitement to us to seek for some other method of remedying the evil" (p. 11, Preface).

But I thought it possible the blessings of *fish-oil*, and his knowledge of the results of *Dr. Williams's practice* (!) might have inspired him with greater confidence in "*the value of medicine.*" But it seems not. *He* knows, no doubt, the difference between *one in eight* and "*eight per cent.,*" and he knows, too, the trick of "*doctoring the statistics!*"

The Lord Chief Justice. I want to see that I quite understand you. In the first place, with regard to the action of imperfect respiration, or imperfect respiration with imperfect atmospheric air—in either case, whether the imperfect respiration arises from some derangement of the air-passages or respiratory organs, or whether it arises from the imperfect nature of the air inspired, you get an impure condition of the blood?—Yes.

That is the fact, that you get an impure condition of the blood?
—Yes.

A certain amount of oxygen, as much as is taken in by a healthy subject, of wholesome air, is necessary to the proper oxygenation, and therefore purity of the blood?—Yes.

Therefore, if you have either imperfect respiration, or bad air taken

in by the act of inspiration, you get the blood in an imperfect condition?—Yes.

Do I understand that, in your opinion, if the blood is rendered impure by either of those two causes, that the impurity will not of itself produce tubercular disease of the lungs?—That is my opinion.

But it is necessary that there should be a previous disposition to the disease, or a scrofulous habit of the subject, in order that imperfect respiration, or imperfect air, should produce tubercular disease?—That is my opinion.

This was his opinion on this particular occasion, but it was not in accordance with his written opinion, for he attributes the disease to "*chronic inflammation*" in his book on the chest (p. 162). He must believe that chronic inflammation, by impairing the function of the lungs, will produce tubercular disease, or how could it be the cause of tubercle? Then mark, he distinctly says that "bad air taken in by the act of respiration" will "get the blood in an imperfect condition," but will not produce tubercles. Now read the following, from his *Principles of Medicine* :—

"The habitual want of pure air especially exerts an unfavourable influence on the state of the blood, and on the functions of circulation and nutrition, causing pallidity of the surface, and imperfect development of the corpuscles and plasma, which then, instead of contributing to the nourishment of the textures, degenerate into scrofulous and tuberculous matter, whose deposition in the internal organs," &c. (p. 53-4.)

Here he attributes tubercles *directly* to the very cause which he says in his evidence will not produce them! This is the evidence of his own book, and there is no escape from the inference that he has either a very treacherous memory, or a very elastic conscience.

Then there is another question. Suppose that oxygen were brought by artificial means into contact with the lungs, is not a healthy condition of the lung necessary in order that the oxygen brought into contact with the lung shall be conveyed to the blood, and so effect the healthy condition of the blood?—Yes, my Lord, to a certain extent. If the condition of the lung is intrinsically disordered, even if the oxygen is there, it may not penetrate into the blood, or the blood may not circulate in the blood-vessels.

That is what I want your opinion about. It is here proposed to apply, by artificial means, oxygen to the lungs. Now, I want to know whether, if the lung be in an unhealthy condition, the presence

of oxygen there, even if the lung could pass it into the blood, would have any effect or use?—No; if there be an obstruction of the passages, the oxygen is of no use.

But then, if there *was* obstruction to the passage of the *oxygen*, there would also be an obstruction to the passage of air, and a patient would hardly get on well without breath. And if the *quantity* of air was diminished by an obstruction, that would only be an additional reason why it should carry more oxygen in less bulk. But his Lordship and the Jury knew nothing of this, so they stored up in their minds as something valuable what was in reality no more, nor any less, than a little *joke* at the expense of their ignorance of medicine, indulged in by Dr. Williams while under the solemnity of an oath. Had it been told with the intention to deceive, it would have been simply untrue; but being sworn to, it was, to say the least, very nearly approaching to something worse.

Is the healthy condition of the lung essential to the introduction of oxygen in the animal economy?—I can scarcely say that. I think that the oxygen may be administered in a healthy state of the lung; and I can quite conceive the possibility of a condition in which the administration of oxygen may be beneficial. If there is internal disease of the lung, or rather imperfection, a sufficient amount of oxygen cannot be conveyed by ordinary atmospheric air—then adding a little oxygen may be of service; but, as I before stated in my examination, those cases seem rather exceptional than common, tending rather more to remove asphyxia.

I gather from what you say, that if you have, by artificial means, oxygen brought into contact with a diseased lung, the result may be beneficial?—It may sometimes, under limited circumstances, be beneficial.

What do you mean by limited circumstances?—There may be circumstances in which the presence of an excess of oxygen may be injurious.

Do not assume the excess; the hypothesis assumes that the necessary quantity of oxygen cannot be brought into contact with the lung on account of the imperfect respiration, which arises from some cause or other?—Yes.

And in that case there cannot be a sufficient quantity of oxygen, and the artificial introduction of oxygen into the air-passages, so as to bring it into contact with the diseased lung, might be beneficial to the diseased lung?—I think it quite possible it might.

DR. JAMES RISDON BENNETT, *sworn.*

Examined by Mr. FITZJAMES STEPHEN.

Are you a Doctor of Medicine?—Yes.

And a Fellow of the College of Physicians of London?—Yes.

And I believe you were Censor in 1857 and 1858?—Yes.

Are you also Physician and Lecturer on Clinical Medicine to St. Thomas's Hospital?—Yes.

And to the London Hospital for Diseases of the Chest?—Yes.

And you are the author also of several medical works?—I have not written much.

Have you read this work of Dr. Hunter's?—I have.

The passages have been read already, and I will only just mention them. Have you read a passage on the cause of tubercle, at pages 37, 38, and 39?—Yes.

Have you also read a passage at page 78?—At the bottom of page 78, the last paragraph?

Yes.—Yes.

At page 87 about tubercles being the fruit of imperfect respiration?—Yes.

And the paragraph in the middle of page 105—"Tubercles are nothing more than depositions of carbon in the delicate tissue of the lungs"?—Yes.

Do you agree in the theory that tubercles are produced, with reference to all those passages respecting Dr. Hunter's theory, by a deposition of carbon on the lungs, and that they are nothing but deposits of carbon on the lungs caused by an obstruction to the oxygen which passes into the lungs in the course of respiration?—I do not; it is contrary to all my experience.

I understand you that there are classes of cases in which great obstruction to respiration exists, and in which there are no tubercles formed?—Yes; there are many diseases with which I am quite familiar in which there is a persistent obstruction to the entrance of air into the lungs—when I say persistent, I mean lasting many months or years—and it is not the experience when those patients die, whatever may be the cause of their death, to find tubercle in the lungs in such cases.

What diseases belong to that class?—Well, I would mention the disease which Dr. Williams referred to, emphysema of the lungs, which is popularly understood as chronic or persistent asthma, long-standing cases of chronic bronchitis, with considerable thickening of the mucous membrane, and obstruction from that cause as well as from the amount of mucus collected in the bronchial tubes, and certain diseases of the heart which are attended with a persistently engorged state of the lung.

Are you acquainted with other diseases in which an extensive deposit of carbon takes place in the lungs, yet in which no tubercle occurs?—Yes; there is a disease, or perhaps I should say there are two distinct varieties of disease of the lungs that have been termed black disease of the lungs, in which considerable quantities of carbon are deposited as black masses.

And that does not produce tubercle?—That does not produce tubercle; tubercle is not found associated with it.

It has not been found in those lungs at all?—Not as a general rule. As far as my experience goes, and I have very accurately examined with reference to that particular question, no tubercle was found at all.

I shall not attempt to follow this witness through the same ground taken by Dr. Williams. It is quite clear that each had his part to play in the farce, while all were to support and back up the evidence of the first witness.

Here, however, is a novelty; and it would be a pity not to reward the ingenious witness who was the parent of it. Because in the black lung there is carbon and no tubercles, *ergo*, carbon cannot be the cause of tubercles. Now, what is the “black lung?” It is caused by the inhalation of dust and smoke in coal and other pits. By many authorities it is supposed to be due to the inhalation of the smoke of the lamps; the soot, little by little, accumulates in the lungs, and the poor miner soon falls ill and shortly dies. This is the usual course; but it is as absurd to say that because soot is deposited in a man’s nose and throat, without tubercles following, that therefore he can have too much carbon in his blood with impunity. But many of these cases do die of consumption; and, for anything Dr. Bennett can know, the immediate cause which produces *tubercle* is the presence of the soot in the air-tubes, which, though not a part of the body, yet obstruct the lungs and render respiration imperfect.

But why did Dr. Bennett not go on to speak of the *second* variety of black lung—that in which the deposit comes directly from the *blood*? But no—he said just enough for the purpose he had in view. He left the same impression on the Court that he would on the mind of any person now reading his evidence, viz., that the second form of black lung did *not* depend on *imperfect respiration*, was *not tuberculous* in its

nature, and was not in any way connected with *tubercles*. This was certainly the impression made upon the minds of the jury and it was clearly the *intended* impression. And yet it is an inference as utterly opposed to truth as any inference could possibly be. But mark, Dr. Bennett will now say that he never said the second variety might not depend on a vitiated state of the blood, and might not be tuberculous in character; read over the *three* last answers given by him and you will see clearly that no other construction can be put upon his replies than that both varieties were covered by them—that neither had any possible connection with tubercles or were in any sense dependent on the tuberculous state of the blood. Now read the following.

Sir THOMAS WATSON says—

“It is never confined to one part, but pervades several; resembling in this respect both the *scrofulous* matter which constitutes *tubercle*, and the matter of *cancer*. Müller, indeed, considers melanosis to be a variety of cancer.”—p. 227.

Again—

“Certain pathologists imagine that in these cases there has been an undue accumulation in the blood of carbon. The changes to which the melanotic tumours are liable are very much like the changes which tubercle is apt to undergo.”—p. 228.

ANCELL says—

“It is at present most generally regarded as modified *tuberculous* blood in the first instance stagnating in the vessels, and then extravasated.”—p. 158.

Again—

“The tendency to melanotic deposits depends primarily upon the condition of the blood . . . hence, in tubercular consumption, it appears to be intimately connected with the disturbance of the respiratory function [impaired respiration].” —p. 159.

Professor SWETT says—

“This pigment [black melanotic matter] is also found surrounding *tubercles*, as the *gray granulation*, the *chalky tubercle*, and *tuberculous excavations*. It is often found in abundance in the bronchial glands. It is [sometimes noticed in the mucous membrane of the intestines, and especially around tubercle of the peritoneum. It is a carbonaceous substance.

Again speaking of the “*expectoration in consumption*,” when examined under the microscope, he says there is mixed with “mucus,” and “saliva,” and “epithelial scales from the mouth,” the following: “epithelial scales from the bronchial

tubes, mucus vibriones, blood globules, crystals, *black pigment*, globules of fat, and granular, and pus globules."

So that this matter which Dr. Bennett thinks is unconnected with tubercles is, according to Professor Swett, a part of the expectoration of every consumptive case, and throughout the body is found mingled with and surrounding all forms of tuberculous deposit. He also believes its abundant secretion to be an indication of the chronic character of the case, for he says :—

"In very chronic cases you will find these cavities lined, &c., secreting very little purulent matter, surrounded by portions of lung of a grayish or reddish hue, and condensed by chronic inflammation, presenting, perhaps, chalky concretions with a *copious of black pigment!*"—p. 220.

Dr. W. H. WALSHE, one of the physicians to the Consumption Hospital, speaks of it as follows :—

"The retrograde anatomical changes of phthisis [consumption] whether excavation have or have not occurred, are *generally* accompanied with more or less copious *deposition of melanaic granules* [black carbonaceous matter]."—p. 432.

ANDRAL believed that—

"The deposition to the formation of tubercles so marked in youth was subsequently replaced by a disposition to the secretion of melanotic matter."—(Ansell, "Tuberculosis," p. 159.)

Now all these facts show that this *carbonaceous matter*, which Dr. Bennett thinks entirely *unconnected with tubercles*, and perfectly innocent, is closely identified *with* tubercular matter, is generally mingled in the expectorated matter of consumptive patients, is often found lining cavities from which the tubercles have been discharged, and is frequently mingled with and around tubercular deposits! It is believed by many authors that its occurrence indicates a tendency in tubercles to undergo a curative change—the solid carbonaceous matter being the residue or detritus of the tubercles—the other elements being absorbed. It is often found in old people who were probably tuberculous in youth.

I have thought it well to show the unaccountable *ignorance* manifested by Dr. Bennett in regard to this matter, or his careful evasion of the truth. He is at perfect liberty to select whichever horn of the dilemma he may prefer.

Now, have you known cases in which respiration was perfectly free but in which tubercle is found?—Well, free as far as we can judge by the ordinary symptoms. If there is tubercle in the lungs, there is a certain amount of obstruction to the respiration.

The tubercle itself would constitute an interruption to the respiration?—Undoubtedly.

Has that been found to exist where there was no mucous or other obstruction to the entrance of the air into the lung, except what was caused by the tubercle?—Certainly.

Where all the air-passages were free?—So far as we could judge.

Not a word about imperfect respiration from *impure air*, or from a *cramped position of the body*. In my book I pointed out *these*, and many other influences which produce precisely the same effect on the blood as mechanical obstruction in the air-passages would. You might *not* have the latter, but *having* the former, imperfect respiration would still exist as the cause. All this Dr. Bennett evaded, and so misled the Court and the public.

And do tubercles occur in other organs besides the lungs, which the air does not reach at all?—Unquestionably.

The heart and the brain?—Yes.

And other organs of the body besides the brain?—Yes.

The Lord Chief Justice. Tubercle occurs in the liver, does it not?—Yes.

Where you find tubercle of the lung, is it common to find tubercles in other parts of the body?—It is; we generally look for them in the other organs, if the patient be above the age of puberty.

Mr. Stephen. Do you agree in Dr. Williams's opinion, that, although imperfect respiration might exercise an injurious effect on the general constitution, it would not alone produce tubercle?—I think we may take it as proved as a matter of fact, not of opinion, that it is not a sufficient cause, because the cases I have alluded to are not rare cases; they may be collected in hundreds, and the general rule is, that there is no tubercle found in those cases. I may say, that so much is that the case, that a celebrated pathologist, Rokitsansky, who has probably had greater opportunities of making post-mortem examinations than anybody living, was so impressed with the fact, that he considered a carbonaceous—that is to say, a venous state of blood, persisted in for any length of time, was antagonistic to the existence of tubercle. Whether that opinion be correct or no, I do not know; but about the fact, I apprehend there can be no question.

Mr. Serjeant Parry. What fact?—That out of a vast number of such cases, tubercle is not found; and those are cases where the pa-

tients have been for months and years breathing imperfectly and circulating a carbonaceous or venous condition of blood.

Mr. Stephen. What was the date of Rokitansky's work on the subject?—It passed through a number of years, but a complete edition of his work was published about ten years ago in this country.

Why, when he was giving the ideas of Rokitansky, did he not give the experience of Dr. Chambers, who found that “of 2500 cases recorded in the *post-mortem* books of St. George's Hospital, the liability to tubercles seemed to be in proportion to the degree of venosity in the part?” (Williams's “Principles,” p. 478.) This is the very opposite doctrine, and teaches us that the impure venous blood in the lungs *does tend* to lay the foundation of tubercles.

Why did he not mention *Dr. Walsh's* opinions, which he knew to be the very opposite? Nay, why did he not say that his dear friend, Dr. Cotton, repudiated the very doctrine he was laying down apparently for the sole purpose of misleading the Court? Hear what Dr. Cotton says:—

“The inaccuracy of this supposition was well illustrated by a patient lately “in the Consumption Hospital, under the care of my colleague, Dr. Cursham. “. The autopsy exhibited the following combination: extensive tubercular cavities in both lungs, with congenital malformation of the heart.”—p. 50-1.

And in the face of this, Dr. Risdon Bennett had the *effrontery* to put forward Rokitansky's opinion as a fact “*about which there could be no question.*”

Now, we come to the question which has been discussed about the chemical constitution of tubercles. You have read the passage in Dr. Hunter's, in which he states—“What does chemistry tell us of the essential nature of tubercle? The analysis of Scherer proves that 54 per cent.,” and so on. Have you heard Dr. Williams's statement on that subject?—Yes

And are you acquainted with Scherer's analysis?—I have seen it in works on this subject, if I recollect rightly.

And do you agree with what Dr. Williams stated about it?—Yes. I do not know whether, after all, his explanation, which was the correct one, was understood; but when Scherer says 54 per cent., he means 54 per cent. of the whole quantity of tubercular matter was carbon, and the remainder of that is not made up of the elements of disintegrated tissues of the lungs, but of the other essentials going to form the tubercle and the tissues as well. The 54 per cent., I apprehend, refers simply to the whole amount of carbon in these tissues in question.

Then this disintegrated tissue is mixed up, I apprehend, with the tubercle, and not separated from it?—Well, it would be very difficult even to separate tubercle entirely from the tissue. Sometimes it might be to a small extent.

But the tubercular matter itself, does it contain free carbon, or is it carbon in an organic state or in connexion with other matters?—In organic connexion with other elements.

Then would it be correct to describe it as pure carbon, or does it, in the sense that you understand it, contain carbon?—All chemical substances contain a portion of carbon. Carbon forms a large part of the whole animal frame.

Is the fact that it is found in that condition, and not as free carbon, consistent with its being a deposit in the lung, caused by the want of oxygen?—Certainly not.

In speaking of the analysis, I say that “*fifty-four per cent. is pure carbon,*” but not that it is *separated* from the other elements except by the analysis. I have not said, nor intended to convey the idea, that carbon, as it exists in tubercle, is separated and exists in its pure state. Had I done so, I must have meant that tubercles were *diamonds*, since carbon is only found pure and separated in the diamond! As I always spoke of it as the *base* or *nucleus* of tubercle, Dr. Bennett could not so grossly misinterpret me except by design.

Do you agree with what Dr. Williams stated about the fact that there is rather a smaller proportion of carbon in tubercle than there is in other parts of the animal frame. There is less, if anything, but about the same, is what he says?—Yes; I would not offer my statement on that subject with any confidence, for I do not carry those matters in my head very well; but the general facts, I am sure, are what Dr. Williams stated.

Now, with regard to letter 7, pages 40 to 44, inclusive, have you read that letter through?—I have read the whole book through.

I will take you, if you please, to the first sentence or two in letter 8: “I have said that in the early stage the most common symptoms of consumption are dry, hacking cough, a sense of shortness of breath on exertion, and increased frequency of the pulse. Another symptom which should lead us to suspect the health of the lung is pain.” Now, having regard to the whole tenor of the previous chapter, look at the passage on page 43, which begins, “If a person supposed to be healthy is found to breathe more frequently than this, when quiet and not aware that the number of his respirations are being counted,” and so on. Do you consider, having reference to the whole matter, that that contains a fair representation? Then it goes on all the way down to page 44, and sums up with five lines at the end: “If therefore you

have a hacking cough and slight shortness of breath on exertion, accompanied by an increase in the frequency of the pulse of ten to fifteen beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal."—I do not agree with that. People go about, and people come to me with a slight, hacking cough, and a slight shortness of breath, who never become consumptive.

You do not consider it right to say you cannot doubt the existence of mischief in the lungs?—Certainly not.

The Lord Chief Justice. With the addition of the acceleration of the pulse?—Yes; I do not agree with it.

Mr. Stephen. May I take it that answer applies to the rest of the letters to which I call your attention?—Yes.

As these are all the symptoms which any authority on consumption lays down as generally present, if we are to disregard them, and treat them as of no consequence, we shall never detect the disease in the early stage, and yet Dr. Bennett will presently tell you that it is only when so detected that cure is at all likely to follow. If the opinion he has expressed on this point gets hold of the public mind it will cost thousands of precious lives in a single year. Its direct tendency, is to make people careless of the early symptoms of the disease and neglectful of treatment until cure becomes impossible.

Do you agree in this, at the top of page 45: "I have said that in the early stage the most common symptoms of consumption are dry, hacking cough, a sense of shortness of breath on exertion, and increased frequency of the pulse"?—Yes, that is so, I think. At an early stage of consumption we frequently have dry, hacking cough, and shortness of breath on exertion.

Do you think they would be clear proofs of consumption?—I do not say that.

Or that, if you have them, you can doubt the existence of mischief in the lungs?—Certainly I cannot say that.

Now, go back to page 10, about catarrh. "Catarrh is the first step towards consumption. When allowed to go on, it involves the throat in granulations," and so on, "and ends in thickening of the mucous membrane lining the bronchial tubes. After this latter change has taken place, we have the deposition of the tubercles, ulceration of the lungs, and the full development of consumption as an almost inevitable consequence." What do you say to that?—I say, as Dr. Williams says, that it is a gross exaggeration, and I think that that is perhaps the mildest term that can be applied to the passage. I have been in prac-

tice a great number of years, and have seen a vast number of cases of diseases of the chest, and I never saw a case of consumption result from this disease of the throat, granular sore throat; and it will perhaps save time if I say the same answer applies to page 15, that elongated uvula, when neglected, commonly ends in a permanent injury to the lungs, and often develops tubercular consumption. I can only say that I never saw a case of tubercular consumption in which I had any suspicion that elongated uvula had anything to do with it, nor do I consider it is likely to be produced by anything like elongated uvula, so far as my knowledge of medicine goes.

He may never have treated a case of *granular disease of the throat*, for anything I know. Those that have, think very differently.

"Pathologically considered, the relation which exists between the fauces, tonsils, "and pharynx on the one hand, and the respiratory tubes on the other, is much "more intimate and important than the connection which exists between the throat "and the œsophagus."

"In almost all the inflammatory affections of the air passages, whether primary "or consecutive, the diseased action has its origin in the fauces and pharynx, and "extends, by continuity, from thence to the respiratory tubes; whilst the membrane "lining the œsophagus may escape inflammatory action altogether, or become but "partially implicated." (Green on "Bronchitis," p. 24.)

Regarding elongated uvula. I have already quoted the authority of Dr. Stokes, Prof. Gross, and others in reply to Dr. Williams, therefore, it is unnecessary to repeat that refutation here.

Look now at the passage about lunar caustic in page 14, beginning "some years ago it became fashionable to treat this affliction by applying to the diseased parts a strong solution of nitrate of silver?"—Do you ask my opinion about the middle sentence, "Fully one half of the consumptive patients whose cases have come under my care, were previously treated for disease of the throat by these caustic applications, and, as they assure me, without any permanent benefit. Indeed, many of them do not hesitate to attribute the disease of their lungs to this practice of burning their throats?"

What do you say to that?—I can only say that it does not accord with my own experience. I do not believe, I do not believe it; and it is not merely with reference to my practice; but, as physician to the Hospital for Diseases of the Chest at Victoria Park, I know the larger proportion of the patients have been under other medical men, many of them coming from special institutions, such as the Brompton Hospital, from which they pass to us, and from us to the Brompton Hospital. It certainly is not true, with reference to the cases that come before us there, that one half of the patients are treated with

powerful caustics before they come under our own observation. I only speak of my own experience.

Suppose a person had disease of the larynx, do you think that cauterizing the throat would be likely to produce disease of the lungs?—No; I never heard of a case of consumption produced by burning a patient's throat, or from burning anything else.

The Lord Chief Justice. Do you see no mode in which it could possibly produce that?—No; I do not. I do not understand how it could. You might assume some excessive example. A patient might swallow a lump of caustic for instance.

That would burn the substance itself?—It would burn away the substance itself, and set up inflammation. I do not know what it might end in.

Mr. Stephen. Then we go on to the passage about the rate of the pulse at page 44?—I agree with what Dr. Williams said about that, that the average rate of the pulse is underrated there considerably.

The Lord Chief Justice. Never under 70 is it in an adult?—Not in an adult. As a general rule, I should rather set the average down as above 70.

Mr. Stephen. In a man?—In men and women together I was taking it. It certainly does not exceed 70 in men, but it is rather above it in women.

The Lord Chief Justice. I understood that the average was 72?—I should think that if men and women are taken together, that is as near as could be.

Mr. Stephen. At all events, is not under 70?—I think not myself.

It varies with position and other things, does not it?—Very much.

Is it a fact that a very slight cause might alter the pulsation 10 beats in a minute?—Yes.

Now we go to page 47 about the young women, the passage that has been read several times: "In young women particularly, I have very often found the lungs seriously affected, while they still retained their colour and plumpness; but, as a rule, if we reduce the matter to a certainty, by weighing, we shall find a few pounds of difference between their present and former weight. If, with the loss of weight, there is a disposition to sigh, a dark discolouration below the eyes and a quickened pulse, with some heat in the hands, set it down as almost certain that the lungs are affected." Have you found that?—No; I think it is very possible that a young woman in a very early stage indeed of the disease, may still retain a great deal of her colour and plumpness. I have seen that, I think; but a little more careful attention will show in the external aspect of such a case that which will excite suspicion of affection of the lungs.

You would not say, at all events, that it happened very often?—Certainly not.

Dr. Bennett here admits the correctness of my observations.

But the Counsel observes, you would not say that "*it happened very often.*" What was this but an attempt to convey to the jury the impression that I had said it was a usual occurrence. I spoke of *loss of flesh* as a usual symptom in this disease, but added, that in young women about the age of twenty they often *apparently* retained their colour and plumpness, but would be found to have lost weight if submitted to actual weighing. It is a mere fact in the history of the disease, and no careful writer has failed to comment upon it.

What do you say to the next sentence: "If we reduce the matter to a certainty, by weighing, we shall find a few pounds of difference between their present, and former weight?"—No; it is not all unfrequent, I think, to meet with young women losing considerably in weight, and having very many, at all events, of the symptoms detailed here, with perfectly sound lungs, and who never became tubercular.

"If with the loss of weight, there is a disposition to sigh, a dark, discoloration below the eyes, and a quickened pulse, with some heat in the hands, set it down as almost certain that the lungs are affected." Would you set it down under such circumstances as almost certain that the lungs are affected?—I most certainly should not.

There is another passage very like that at page 71, which comes under the same head, the last paragraph. "I always suspect the health of the lungs in children when I observe them subject to any of the following symptoms: Eruptions about the face, puffy and chapped lips, pastules on the edges of the eyelids, eruptions behind the ears, soreness and running from the nose, swelling of the glands of the neck, and gradual wasting of flesh and strength. None of these are necessarily signs of consumption, but they show a bad state of the general health, which in a great many cases, will be found to arise from the presence of tubercles in the lungs. An examination will always put the matter beyond doubt, and consequently that parent who neglects to have it made is guilty of great cruelty and neglect." Do you agree with that?—I do not agree that those symptoms arise from the presence of tubercles in the lungs. I think you may meet with children who have these symptoms which are detailed more or less. That they show a general bad state of health, and that those children on examination would be found to be tubercular is very likely I think, but I should not take those as arising from tubercles in the lungs.

They might have them, but you should not consider them as evidence of tubercle?—No, still less as arising from the presence of tubercles in the lungs.

And you would not think that parents who neglected to have their children examined were guilty of great cruelty and neglect?—I think

any parent who had his child in that lamentable condition represented here, would be guilty of cruelty not to have him examined.

But you could not necessarily consider that they were symptoms of tubercle?—No, certainly not.

You would have a general examination, I suppose, if you found those symptoms?—Yes.

Now, turn to page 111. “As in this climate most cases of consumption arise from colds which have become seated”—do you consider that most cases of consumption arise from cold which have become seated?—No, I do not.

Here he differs from Laennec, who tells us that he believes the great majority of first cases are mistaken for colds by careless, incompetent physicians.

Laennec, says—

“We may indeed say that the greater number of cases of consumption are *latent* ‘at the beginning, since we have seen that nothing is more common than to find ‘numerous miliary tubercles in lungs otherwise quite healthy, and in subjects ‘who had never shown any symptoms of consumption.”

Again :—

“Since I was first led to adopt this opinion on anatomical grounds, it has frequently appeared quite clear to me, from carefully comparing the history of my ‘patients with the appearances on dissection, that the greater number of those ‘first attacks are *mistaken for colds*, and that others are quite latent, being *unaccompanied with either cough or expectoration*, or indeed any symptom sufficient ‘to impress the memory of the patients themselves.”—p. 334.

Sir James Clark writing on the same subject says :—

“I am satisfied, from my own observation, that Laennec’s opinion is correct. “. . . . The cases of this kind which I have observed have been most frequently ‘in females.”—p. 143.

Even Dr. Cotton, one of Dr. Bennett’s partners in this business of giving evidence against me at £25 a day, admits that his patients attribute their diseases to attacks of cold.

“*Influenza and Catarrh.*—The frequency with which consumptive persons attribute the commencement of the disease to ‘an attack of influenza,’ or ‘a severe ‘cold,’ at once points to the influence of these disorders in the development of ‘tubercle. Yet, I believe that neither the one nor the other, however severe or ‘lasting, is of itself capable of originating phthisis ; or that persons who have suffered from either, are, on that account, more liable to become phthical. It is ‘only when superadded to a strong tuberculous predisposition, or when attacking ‘persons already the subjects of latent tubercle, that their connexion with Consumption is at all apparent. Under either of these circumstances, however, influenza acts most unfavourably, by developing or accelerating the tubercular

“disease : and many of the most intractable cases of phthisis have appeared to me to have thus originated.

“Catarrh, also, when severe, and accompanied with constitutional disturbance and bronchial irritation, appears, in many instances, to bring into activity phthisis hitherto dormant. Their prejudicial operation is not difficult to account for. In influenza especially, but also, to a less degree, in severe catarrh, are associated the very conditions most likely to arouse or accelerate pulmonary tuberculosis ; the general health is reduced, whilst, at the same time, the bronchial mucous membrane becomes morbidly irritable and congested.” (Cotton, on “Consumption,” p. 92-3.)

It is all very well for these gentlemen to “*assume*” that the patients must have been ill before. The patient says no ; I took cold, it settled on my lungs, became chronic, my breath became short, and now I want to know what is the matter with me. Dr. Cotton examines his chest, and finds his lungs filled with tubercles. Still, he won’t believe that the cold really produced them. Now, of what consequence is that to the patient? But for the cold he would have remained in health. Whether the cold produced them *de novo*, or merely developed them into fatal activity, secondarily, is not worth a straw to him. If he takes a cold which becomes chronic, and hangs about him for months, is it not his duty to himself and to his family to inquire carefully into its nature? Is it not the duty of the physician to point out to him that it *may arise* from tubercles, or may *kindle* them into activity? If there are tubercles, every hour is precious time, and a month’s delay may carry him into active Consumption ; if there are none, he will not be made the less happy by having his lungs examined, and knowing that there is no danger.

Then it goes on, “it will be understood that in all such cases we have a chronic inflammation of the lining of the air passages, nasal catarrh, sore throat, and chronic bronchitis, all exist, in greater or less severity, and the physician must remove them and bring back the mucous membrane to health before the function of the lungs can be restored. If they be permitted to remain they will continue to obstruct the bronchial tubes with mucous, diminish their size, shorten the breath, and prevent a sufficient quantity of air from being received to keep the blood pure. They are in fact the real cause of consumption, and so long as they remain they feed and increase both the carbon in the blood and the tubercles in the lungs.” From what you have said already, I presume you do not agree with that?—No, especially

the last sentence there, inasmuch as I cannot look upon them as the real cause of consumption.

The object I had in view in warning the public against these diseases, I have explained sufficiently in my book.

Sir Thomas Watson says :—

“I have no doubt whatever that the dormant predisposition is often awakened into actual disease, and that latent tubercles are often accelerated in their progress by inflammation of the pulmonary tissues. Whether this happens directly from the local inflammation, or directly from its effect in lowering the vital powers, is a question which no one can solve, and of which the solution is not of much consequence. What we are sure of is, that every one who bears a real or suspected taint of scrofula in his frame, should scrupulously guard against every known and avoidable cause of catarrh, pneumonia, and pleurisy. I hold M. Louis’s doctrine on this head to be unsound and unsafe; and I mention it only to admonish you against it.” (P. 206—7.)

“From these facts, the reader can understand that one of the great objects of treatment is to subdue this catarrhal condition of the mucous membrane. How can this be accomplished? It is no treatment for a disease in the lungs to pour cod liver oil and tonics into the stomach, for they never reach the part affected; and besides, such medicines possess no properties capable of effecting cure if they did. No physician will pretend that cod liver oil, or any cough mixture, or tonic ever compounded, has power to remove tubercles, purify the blood or heal the mucous membrane, even if directly applied. How, then, in the name of reason, can they accomplish these objects, when they are applied to a distant and healthy part? They are merely palliatives, nothing more; and nobody, unless it be the patient, expects them to heal the lungs.” What do you say to that passage?—Well, I say, if tubercular consumption is a disease depending on the derangement of the whole system, impaired nutrition—a degraded state of the system, as Dr. Williams has called it—then I think remedies taken in by the mouth and by the stomach are important to regulate the system, and that direct agents may be introduced, which, passing into the blood, may produce a very considerable amount of effect on the lungs. I think that even by diet alone you may do a great deal towards relieving, and I may say, in favourable circumstances you may even cure tubercular consumption without any medicine at all. I go so far as that. Doubtless those cases are very rare and exceptional, but what I mean is this, that if the constitutional predisposition is very slight, and consumption has been induced in consequence of a patient’s being placed in very unfavourable circumstances as regards living and so on, removing them out of those circumstances, putting them upon appropriate diet, and sending them out of the country would be all that is necessary to cure and arrest the disease; or, in other words, to cure tubercular consumption. Well, *à fortiori*,

it is reasonable to suppose that remedies of the kind mentioned here—I do not mean these alone, but various other remedies calculated to act on the digestive and other organs, the liver and the lungs—would be likely to exercise a beneficial influence on the lungs and I believe they do. I believe that cod liver oil, and steel, and various other agents that may be mentioned, have a direct influence in controlling the tubercular deposition. It is perfectly true that this tendency is so strong in the system in too many cases that all our remedies are unavailing, and the disease pursues its course to a fatal end, notwithstanding all this.

All this has very little to do with the point submitted to Dr. Bennett. He runs into a general eulogium of medicine by the stomach, and thinks they “*may*” produce a considerable amount of effect on the lungs, “and under very favourable circumstances *may even cure consumption.*” Still he is compelled to admit that the result is “*rare and exceptional.*” This is precisely what I have said in regard to them. Cure by such means is “rare and exceptional!”

Do you consider that cod liver oil and tonics introduced into the stomach might or would reach the lungs?—I believe that virtually they would reach the lungs in a vastly more effective way than any remedies applied to the lungs themselves, because I believe they would be more likely to act upon the blood, and in the action upon the lungs by the blood the other parts of the body would participate in.

It is clear, then, that you do not agree in the doctrine of the utter worthlessness of those medicines which effect a cure when directly applied, if they are administered through the stomach?—No; I certainly do not, and it would be only necessary to look at the records of any well regulated public establishment in London; for instance, take the Brompton Hospital, or take any other large hospital, and I think you would find in those records sufficient proof to show that a vast degree of good was effected by such means.

I am afraid the *good* is “*rare and exceptional*” and the *harm* the rule. It is clear that Dr. Bennett has not studied the statistics very carefully, or he would know that where $29\frac{1}{2}$ died from diseases of the lungs twenty-five years ago, now $36\frac{1}{10}$ die. If this fact is proof of “*a vast deal of good,*” then it is of a kind which few would care to have applied to themselves. The chances of life are few enough in this disease without having them diminished by a persistent reliance on fish oil!

In page 113 there is a passage which says, "The medicines which it is necessary for the patient to inhale are of four kinds:—First, expectorants, to expel the mucous; second, sedatives, to allay irritation; third, astringents, to diminish secretion; and fourth, alteratives, to change the action of the diseased membrane." Would that classification of medicines enable you as a medical man to know the remedies which Dr. Hunter proposes to use?—No; I should say that that was a very indistinct and unsafe guide for him. Of course, I have some notion of what he means by expectorants; I have some notion of what he means by sedatives, but I do not know what he means by inhaling them. I will say at once that I read this book before this case was likely to come on, or before it was likely that I should appear as a witness at all, and I could not make out what his system was; it seemed to me, taking the book as a whole, from beginning to end, that he professed to cure consumption by introducing oxygen into the system through the lungs. Then, naturally, I looked to see how he procured his oxygen, but there is no reference in the book at all how the oxygen can be procured; or, at all events, as far as my memory serves me, nothing which would enable me to carry out the object of the author of this book.

Or how to introduce it into the lungs if you had procured it?—No; nothing to show how to introduce it. There is the inhaler spoken of, but there is no special inhaler described in any portion of this book, is there? My memory does not serve me as to that point.

Can you say whether there are any instructions as to how to medicate vapours or how to administer medicated vapours, or what vapours you were to administer?—I do not recollect any description in the book of that kind. There is one passage somewhere towards the end, which has not been referred to, which puzzled me a great deal.

Mr. Karlake. We will come to that by-and-by.

Mr. Stephen. Supposing oxygen is to be used as a remedy, would it be an expectorant, astringent, sedative, or alterative?—I really do not know very well how it would be likely to act, except that I think with Dr. Williams, that there is some evidence of its acting as an irritant or stimulant to the lungs; at all events, what little one does know of it would lead me to abstain from giving it where there was liability to hæmoptysis, or bleeding of the lungs. I should not like a patient to try inhalation of oxygen in such a case as that.

Considering the "*little*" Dr. Bennett pretended to know of oxygen, would it not have been as well if he had "*abstained from giving*" any opinion. Had he understood anything about oxygen, he would have known that it does not excite, but, on the contrary, produces a soothing and sedative effect through the relief it affords to the tense capillary vessels of the lungs.

The Lord Chief Justice. I understood that the way in which he expects the sedative effects of oxygen introduced into the system by inhalation are to take place, is by passing it into the lungs through the circulation. I do not know whether it is explained in the book, but there are statements that have reference to that. Imperfect respiration, he says, causes carbonization of the blood, and you can only get rid of the carbon by oxygen artificially introduced, because imperfect respiration prevents the necessary quantity from passing into the circulation. That is what I take to be his theory?—Yes; and that is what I took at first to be his theory, but on looking at some passages in the book they appeared to me not to carry out that view. For example, at page 111, he speaks not only of restoring the purity of the blood, which may be done in the way you mention, but he speaks of the oxidation and absorption of tubercles, as though he imagined that the oxygen being brought into direct contact with the tubercle was capable of oxidizing it, which is a perfectly unintelligible statement to me. I should like to hear the opinion of any competent chemist on the subject, but as far as my knowledge of chemistry goes I should say that that is impossible.

If you could lay the surface of the lung bare, and bring the oxygen into contact with it, you do not think it would produce any healthy effect?—I go further than that. Even if you take it out of the body and immerse it in any quantity of oxygen.

Mr. Serjeant Parry. You are speaking now with reference to the statement that you must begin “by restoring the purity of the blood and causing the oxidation and absorption of the tubercles?”—Yes; I refer to that.

The Lord Chief Justice. The question is whether if the oxygen is brought into direct contact with the tubercle it will get rid of the tubercle?

Mr. Serjeant Parry. That, I do not think, he has borne out.

Mr. Stephen. You had reference, in reading the book, to the passages in which he speaks of tubercle being deposits of carbon?—Yes.

Is there not a passage in which he says there is nothing else that could take it out?—Even then, supposing tubercle to be pure carbon, which we know it is not, I do not understand how, bringing carbon into combination with oxygen as a gas, without any other aid, would oxidize it. I speak under correction.

Of course, if tubercles were “*diamonds*,” which they are not, oxygen would not combine with them; but if they are largely made up of *hydrocarbon*—that is to say, of *oil* and *cholesterine*, then oxygen *will* combine with them.

Liebig says—

“Wool, hair, or cloth impregnated with these compounds [fats and oils] absorb oxygen from the atmosphere with great avidity.”—p. 33.

On this point, at least, I prefer the opinion of Liebig. But all this evidence is mere sham. Dr Bennett and the other witness for the defence, first lay a false basis by construing me literally ; they take carbon, as used by me, to mean *dry, inorganic carbon*, and on this transparent sham they build their evidence. As they must have known that I had no such meaning, their evidence was trifling with the Court, and yet the Court could not see that such was the case. They were enjoying a joke at the expense of the Chief Justice and the jury ; and, no doubt, had a good laugh over it after they had done !

Mr. Karlake. In page 114 he says, "It is frequently asked, 'Do you give no medicines by the stomach ?' Certainly I do,—when they are required." Then he gives certain cases, and in the next page there is this :—"Unaided by inhalation they never yet effected the case of a cure of consumption. Combined with the direct application of oxygen to the tubercular deposits and the blood, and with the daily inhalation of medicines to act upon the diseased air tubes and cells. I read that as meaning you break up the tubercle by putting oxygen upon it?—Yes ; I knew there was more than one passage in that book which seemed to bear out that theory.

Mr. Coleridge. If you read on you will find the last passage is : "For together they accomplish more certainly that end for which the patient submits his case to the physician—the healing of the lungs."

Mr. Stephen. So far as your chemical knowledge extends, if a man did assert that tubercle could be decomposed or broken up by the artificial contact with oxygen, he would assert something that is not the case?—Broken up is a different statement from oxidizing it ; he speaks of oxidizing it.

Well, would it be possible to oxidize it?—I think not.

By bringing it into contact with oxygen?—By simply bringing it into contact with oxygen.

The Lord Chief Justice. What do you understand by oxidizing?—I understand it to mean bringing oxygen into chemical combination with some other element in the blood.

Here I see Dr. Hunter speaks of oxidation?—Yes, we speak of oxidizing, and not of oxidating.

Is it a scientific term?—No, I do not suppose that we should speak of oxidizing the blood.

Mr. Stephen. A diamond is pure carbon, is it not?—I believe so.

Supposing a diamond were put into a vessel filled with pure oxygen what would be the effect on that?—You had better ask your chemist.

This is taking my words literally, and not in the sense in

which he knew that I intended them to be taken. Carbon in the system exists in the form of fat and other compounds essentially carbonaceous in their nature. Oxygen will combine with the carbon of fat wherever it may find it. When oxygen enters the blood, it combines with fat, and if it found fat in the air-tubes it would combine with it there. Every word of his evidence on this point was an insult offered to the Court, since he knew, that when we speak of carbon in the body, we mean combined with other elements, and that from such combination oxygen will separate it.

Is there anything in the book of Dr. Hunter which enables you to ascertain what are the sedatives or astringents which are to be used, in what form they are to be used, in what proportion, or in what cases?—Not in what form or in what proportion; only generally in what cases. I think stramonium is mentioned somewhere in the book.

Mr. Karlake. No.

Mr. Stephen. Now I will ask you another question about it. There are a considerable number of cases described in this book, English cases numbered from 1 to 14 after the preface to the first edition. Have you read those?—I do not know whether I ought to say I have read all those, but I saw such a strong similarity in them that I slipped a good many of them.

Are any of them described in such a manner as to give you any information as to the mode of treatment?—No. I think they are utterly worthless to a medical man so far as I can judge.

Have you also read the passage at the bottom of page 130 about the oxygen chamber?—That is another passage which I was going to refer to. I do not understand that; it puzzled me much when I read it. I supposed at first that he had a chamber specially prepared into which he could admit and allow the exit of any air he pleased, so as to retain the air in the chamber of a certain definite constitution; and into that he might have admitted a certain amount of oxygen, and it is quite clear that that is a thing which could only be carried on at his own house, or under special arrangements; patients would not be able to do that themselves at home, and yet there are passages in the book which seem to indicate—in fact this very letter that precedes that statement seems to indicate that it was done. This statement says at times there was only an addition of one-third, while in others one-half or two-thirds in this chamber. If that be so it would require a person very much acquainted with the matter to produce a material in this chamber, and regulate how much *per diem* a patient was to breathe. I do not understand how that could be carried out.

Now there is the case of Joseph Warford, Esq., in a letter addressed to the *Troy Daily Times*, at page 128. I take that as a specimen, because there are various letters from persons living at Niagara and elsewhere? Have you read them?—I think I did.

Are they of any use to a medical man?—It is some time since I read those.

I am sorry he thinks the cases published in my book worthless. They are too favourable to me for his purpose. I can only regret he has not written a book on the lungs himself. We should then, doubtless, have some *good* cases. The only book he has published is one on Hydrocephalus, from which I learn that he had *four* cases of that disease, *two* of which he had the satisfaction of examining after death!

Mr. Karlake. You see this in the letter of Joseph Warford: "I inhale in two ways. Warm from an inhaling instrument, and cold by filling a chamber with vapour, into which I go three or four times a day; remaining from five to ten minutes at a time, according to the strength of the vapour."—I cannot understand it myself. Even if I were a patient I should not know how to submit myself to that treatment at all; in fact, I do not see from the book what proof there is, unless it be that elsewhere it is stated, as the patient was put in an oxygen chamber. I do not see what proof there is that oxygen was given. It is stated repeatedly and assumed, but there is nothing in the book which would enable me to say with confidence that oxygen was really given. Oxygenated vapours and so forth are spoken of, as they have been in the course of the trial frequently. I was present on the first day, and I do not know whether it was Dr. Hunter or some one else who spoke of the various modes of generating oxygen, but I do not think he described any of those processes.

The only process we have heard described at all is, that oxygen might be generated by cloric acid. What do you say to that?—About that, again, I think you had better ask some of the chemists. I never did it myself, and I never heard of oxygen for medicinal purposes being generated in that way.

In your own practice and experience of the disease, have you known of the use of inhalation?—Certainly.

For many years, or how long?—For many years.

What have been the substances inhaled?—Well, I have myself tried a good many chiefly for palliative purposes, but not entirely. I remember, some years ago, trying chlorine and iodine, both of them in the form of vapour, with a view of introducing them into the system, and I found the only way of doing this with any degree of comfort—

The Lord Chief Justice. What did you say you used?—Chlorine

and iodine; and I found that the only way of doing it for the patients' comfort was to let them respire the atmospheric air impregnated more or less with the substances I mentioned.

How did you do that?—By exposing them in the chamber where the patient was.

What was the result of your experience of that mode of treatment?—I never derived in any instance that encouragement that led me to think that much was to be done by it. I think a good deal may be done with it as a palliative remedy, and I apprehend much depends on the symptoms.

What symptoms?—The cough, the difficulty of breathing, and the dryness of the throat.

And asthma?—Yes.

Very frequently?—Constantly.

Bronchitis?—Yes, and bronchitis.

And anything in the nature of coughs or catarrhs?—Yes; it would generally have a sedative effect on the different organs exposed to the inhalation. Many of those things have a soothing effect, and are of considerable advantage to the patient.

Have you tried conium amongst other things?—Yes, frequently.

Stramonium?—Yes.

Chloroform?—Yes.

Creosote?—Yes.

You have tried all those things, and others besides?—Yes, and others besides.

Used you to apply the vapour through steam of water?—Through the steam of water; most of the substances.

The latter part of Dr. Bennett's evidence must have rather astonished his Lordship. No wonder after hearing so much from Dr. Williams of the "*irritating*" and "*noxious*" nature of chlorine, he turned to the witness with evident surprise and inquired, "What did you say you used?" "Chlorine," replied Dr. Bennett. There could be no mistake; he had been using that vile chlorine for twenty years as a "*palliative*"! Only think of that—chlorine to ease "cough," relieve "difficult breathing," and to produce "a *sedative* effect on the different organs exposed to the inhalation." What? an "*irritant*" employed as a "*sedative*," that is to say to *soothe irritation*!

Cross-examined by Mr. SERJEANT PARRY.

You are physician, are you not, to the London Hospital for Diseases of the Chest?—Yes.

You are now?—Yes.

How many years have you been connected with that hospital?—I suppose hard upon twenty years.

During the whole of that time has inhalation been a mode of practice or treatment there with you?—Oh! before that. I remember, as a student, seeing and using inhalation.

And it is a recognised mode of treatment in this country by medical men?—Quite so.

Now, not taking the physicians of hospitals, who are mostly acquainted with the best methods of treatment, do you think that inhalation is used at all as a method of curing consumption, or supposed consumption, or suspected consumption, by the physicians of this country?—Yes; for all diseases of the chest, more or less.

There was nothing at all that was startling or which surprised you in the fact that Dr. Hunter used the means of inhalation?—Nothing at all.

In point of fact, as you say, every educated medical man in this country would use those means?—I should think so, either more or less.

I think I caught an expression from you that they were rather used as palliatives?—Well, I suppose in a large proportion of the cases they would be used as palliatives; but in asthma, for example, they are used by many, and considerable confidence is placed in them by many, as a direct means of cutting short and curing the disease.

Most patients, I fancy, prefer the *shortest possible cut* to health; and honest, conscientious physicians, will adopt the most direct means of "*cutting short*" disease. I can readily understand, however, why some physicians may prefer the *longest way round*, especially when they are to act as guides, and are paid in proportion to *time* and *distance*. Many a sharp cabby has been known to drive his fare round about through tortuous streets and bye-lanes, to confound his mind and furnish a pretext for a double rate!—Those who like the *longest way*, cannot do better than consult Dr. Bennett, the advocate of the practice.

Now, as regards the treatment of consumption and diseases of the chest, the larynx, and so on, through the stomach, do you think it would be preferable to treat them by inhalation, if you could do so, than to treat them through the stomach?—I do not, as a general rule.

That would be a matter of opinion, would it not?—I think not entirely. I think one's physiological knowledge would lead one in a large proportion of cases to prefer to treat through the stomach.

In this book, which you say you have read, Dr. Hunter does not propose to treat any patients who may come to him exclusively by in-

halation.—I should not think any of them, by the general impression made by his book upon me.

Then he does not pretend, as you have read or understand the book, to have discovered any exclusive remedy by inhalation for the cure?—The whole book is based upon the assumption that he has discovered a method of curing tubercular consumption.

Not of curing; treating?—And of curing.

Curing in certain stages, no doubt?—Well, generally, the object of the book clearly is the curing of the disease.

The object is the mode of treatment by which he can do more than can be ordinarily done.

The Lord Chief Justice. By which he can cure the disease.

Mr. Serjeant Parry. Yes; and by which he can cure it. I do not wish to go into the detail of the passages, but I think the tenor of the book is this, that if consumption is to be cured it is to be cured by that treatment, and by that treatment alone, combined with all other scientific remedies which he may avail himself of.

The Lord Chief Justice. “The question is, can consumption be cured? My answer is unequivocally yes, even after tubercles are deposited.”

Mr. Serjeant Parry. Exactly. Allow me to ask you this—I am much obliged to my lord for calling attention to that passage: Are you of opinion that consumption can be cured after tubercles have been deposited, if you can by any means get at them, at an early stage?—I am, with the limitation given by Dr. Williams.

What limitation was that?—In the first place, if the constitutional predisposition or tendency is not too strong to overcome the remedies; secondly, if the disease, which has already invaded the lungs, has not invaded more than a certain portion; and, thirdly, if it has not advanced to such an extent in that portion as to have seriously undermined the general health.

You see in that passage he uses the words even after tubercles are formed. Does not that contemplate the early stage?

The Lord Chief Justice. Surely, the tendency of the whole book is, I rather think they can.

Mr Serjeant Parry. I admit that it is not at all an unfair interpretation of the book.

I hope his Lordship and the jury obtained a clear and satisfactory idea of the extent to which consumption is curable on Dr. Bennett’s plan. Were I a patient, I should endeavour to find out first *how strong* my disease was, and then *how strong* Dr. Bennett’s physie might be. I would thus be able, perhaps, to determine which was strongest, and so decide whether it would be judicious to bring them together. But then there would be that contingent question, No. 2, as to whether the

extent of my lungs involved was "*a certain portion.*" If so, I should, of course, have a better chance, providing always contingent No. 3 did not arise to mar my hopes by revealing the fact that the disease had "*advanced to such an extent.*" Of course, if that was the case, I should know that it was all up with me! I very much fear that one or other of these contingencies always *does* arise in Dr. Bennett's cases, to rob him of *immortality*, and them of *hope*.

There are, of course, before tubercles are deposited, indications of consumptions and disease of the lungs, known to medical men, or is that the only certain mode of knowing that consumption has set in?—I do not think that I quite understand your question.

Before tubercles are deposited, are there any other indications—you can ascertain by the stethoscope, can you not, whether tubercles are deposited in the lungs?—Yes, generally.

In a general way, before any tubercles are deposited on the lungs, there are indications known to science, are there not, by which a person may be known to be consumptive, or have a tendency to it?—That he may have a tendency; but we could not speak of him as consumptive unless he had the tubercles.

But there are indications if he has a tendency?—For example: the child of parents who were consumptive who exhibited in his history or constitution a general delicacy of constitution, I should fear, some day or other, might become consumptive, from the simple fact that he was a child of consumptive parents. That would be enough to justify the fear in the mind of a well-informed man that such a patient might have consumptive tendency.

I do not know whether such cases would be frequent, but such cases have come within your knowledge and experience?—Oh, they are not unfrequent, and sometimes I am disposed to flatter myself that we are in the habit of preventing cases of consumption by acting upon that.

In such a stage of the disease, or suspected disease, should you use inhalation?—No, I should not.

You would treat it through the stomach?—No; I should treat it through the atmospheric air very much myself.

You would prescribe change of climate or change of air?—Yes.

Which is a very valuable kind of inhalation?—The most valuable to us of any.

In every kind of disease, is it not?—Yes.

Dr. Bennett here merely refers to those predisposed, but not actually suffering from any of the forms of local disease described by me. If such a patient had bronchitis, sore throat, nasal catarrh, or enlarged tonsil, or uvula, he would either

treat these conditions, in *addition* to affording the patient the benefit of pure air, or he would neglect his own duty as a physician, and expose the life of his patient to needless peril.

From this book I understand you to say you do not gather that Dr. Hunter uses inhalation alone, but deals with other remedies as well, and treats the patient in the stomach as well as topically?—He says so, but he does not place any confidence in them; they cannot act upon the lungs, they do not affect the consumptive part; they improve the appetite, but they are useless as regards the disease of the lungs; there is still the disease going on there. That is where we are at issue.

Still he would use these means as accessory to any other means?—He says so in words in more than one place in the book.

I presume you had the advantage of hearing Dr. Williams' evidence?—Yes.

And you heard his cross-examination, did you?—Yes.

Now, do you agree mainly with what I may call his evidence or argument?—I do in the main.

Now, there is a passage in page 111 which you have criticised: "I have already explained why no treatment can possibly effect the cure of consumption which does not begin at the foundation by restoring the purity of the blood and causing the oxidation and absorption of the tubercles." Does not he propose there, by means of purifying the blood, to cause the oxidation and absorption of the tubercles?—No; he proposes to do two things as I read the passage.

You said you could not understand what he meant, or how he could oxidize the tubercles, by merely inhaling oxygen, or applying oxygen, that is, locally to the tubercle. Read this sentence:—"I have already explained why no treatment can possibly effect the cure of consumption which does not begin at the foundation"—listen to this—"by restoring the purity of the blood, and causing the oxidation and absorption of the tubercles." Do you read that as two, or as altogether coupled and joined?—I understand the passage to mean this, that he hopes to cure consumption by restoring the purity of the blood, and also by causing oxidation and absorption of the tubercles.

Would not freeing the blood from carbon be a means of restoring the purity of the blood?—*Quod* the carbon it would.

Carbon in itself is a deadly poison, is it not?—No.

Carbonic acid is, then?—Yes, carbonic acid is.

The Lord Chief Justice. Carbonic acid is carbon, plus the oxygen?

Mr. Serjeant Parry. Yes.

You say the blood is purified by getting rid of the carbon; but that would not be the sole means of purifying the blood. I ask you this, would purifying the blood produce at all, or tend to the absorption of the tubercle?—No, not directly to the absorption of the tubercle. If

the blood was thoroughly pure you would probably have no more tubercle deposited from it, other things being equally advantageous to the welfare of the patient.

The Lord Chief Justice. What would be the effect of oxidizing the tubercle?—I cannot understand it. As I have stated, it is perfectly unintelligible, because he does not speak of oxidizing the blood, or any principle of the blood.

He speaks of oxidizing the tubercle?—Of oxidizing the tubercle; and that, I say, is perfectly unintelligible to me. I would submit to any chemist who could inform me better on the subject.

That is the meaning you apply to that passage?—That seems to me the only meaning you can apply to it.

He does not think oxygen introduced could produce oxidation of the tubercles. Now, I supposed it to be one of the recognised doctrines of chemistry, that oxygen would combine with hydrocarbon, and eliminate it.

Liebig says—

“The cause of the different action of oxygen in the body and out of the body is thus rendered clear. In the blood the oxygen is contained not in the gaseous, but in the liquid or solid form, and, indeed, in the most loose form of combination, which allows it to exert in full force its *oxydizing* power.” (P. 32.)

So that the oxygen is absorbed into the blood, and there held in a liquid or solid state, ready to exert its *oxidizing* power, and it will oxidize hydrocarbon whenever it comes in relation with it, whether that be in the lungs or other parts of the body. Then, if we assume that tubercle is poured out on the free surface of the mucous membrane, and that it does contain fat globules and cholesterine, how can he say that it may not be absorbed by them, enter into composition with the hydrogen of the hydrocarbon, and with the liberated atom of carbon, and so eliminate it?—It will even do that out of the body. But this is mere theory, which I cannot prove does really take place, nor he that it does not. It is like Dr. Williams’s *oiling* fancies—mere matter of opinion. What I meant, Dr. Bennett knew perfectly well, was that the oxygen was absorbed by the blood and brought in contact with the tubercles by the blood’s circulation. If it would not reach them through the nutrient vessels, then tubercle must be wholly without vitality, which is not believed to be true. Again, if oxygen, the most subtle of agents, could not reach the tubercles through the blood,

how could fish oil and physic, put into the stomach do so? At best they could only be absorbed into the blood, and then they would certainly be just where it was, without any vital or chemical affinity for the effete carbonaceous waste which we wish to eliminate. All this trash, which the Court and the public have swallowed as evidence, was based upon a fiction—viz., upon the pretended belief that in speaking of carbon in the body, I meant *dry, inorganic carbon*, diamond, charcoal, or plumbago.

Have you used any instrument at all, or are there any known to the profession, by means of which you can inhale?—Yes; it is part of the treatment of the diseases of the chest. By ascertaining the number of inhalers extant, and the amount of trade done in them, because they are sold by thousands and thousands every year of all sorts and kinds, you can tell that.

Have you looked at page 32 of this book, where the instrument is described which he says he uses. That would describe it would not it. “The manner of administering medicines by inhalation, in bronchitis, is as follows: the inhaling instrument, which is made of glass, and which holds about a pint of fluid, is half filled with cold, hot, or warm water, according to the nature of the case.” Are the inhaling instruments you are acquainted with made of glass?—Some of them, some are not.

Some are made of earthenware, are they not?—Yes.

Metal?—Yes.

Any person would know by reading that that he meant the inhaling instrument ordinarily used in the profession for the purpose of inhaling?—I do not know what any other person would think; it conveys no idea to my mind except this, that it is an inhaling instrument, made of glass; that is all I know of it.

But you know of such instruments?—Yes, there are various inhaling instruments, and many made of glass.

And many of them in your hospital?—We do not use them made of glass, but earthenware.

But you have them there?—Yes; and we have some of the glass ones now, I think.

The Lord Chief Justice. Would those instruments, such as you are acquainted with, be available for the purpose of inhaling oxygen gas?—It would depend entirely on the way it was supposed the gas was generated by their medium. If it was supposed that it could be set free by pouring hot water on it, then these instruments would be available for oxygen gas.

I do not know whether you can tell us whether chloric acid would be decomposed by the action of hot water, so that the oxygen gas might

be set free?—Chloric acid, I believe, is rapidly decomposed by a certain temperature—not a very high temperature, I believe; but if it is decomposed in combination with other things, or in combination with water, as Dr. Williams has said, it is manifest that the components of the chloric acid would come off more or less, so that the patient would get the chlorine as well as the oxygen.

Do I understand you to say that oxygen gas might be obtained, but it would be necessary somehow or other to stop the chlorine from going into the instrument?—If I were speaking of curing consumption or any other disease by oxygen, I should assume it to be a first necessity to get oxygen in a pure state, and to get it in such a way as to insure its introduction into the lungs, and I know of no mode of doing that except by collecting the gas in a bladder of a certain kind, so as to take it to the patient's house; and in that way it has been given at St. Thomas's Hospital, but the gas has been manufactured at large in the chemical laboratory of the hospital, and collected in large bladders and carried to the patients' bed-sides, where they have inhaled it.

We understand that Dr. Hunter has obtained it by means of chloric acid?—Yes. By this book I cannot make out how the oxygen is got. There is no evidence in this book that oxygen is given as I read it.

Supposing you had any of these instruments with which you are acquainted, would such instrument be available for the purpose of conveying oxygen generated from chloric acid into the lungs?—They would be available for the decomposition of the gases coming off from chloric acid.

But not of pure oxygen?—Not pure oxygen.

You heard Dr. Hunter say that he makes use of chloric acid?—I heard him or some one say that that was a mode of treatment.

The witness here exercised considerable tact to avoid giving a direct answer. He says, if he wished to treat a patient by oxygen, he would want to "*get oxygen in a pure state.*" Of course he knew that I had said I did not wish to get it in a pure state; ergo, I, who had used oxygen in every possible form during the past fifteen years, could not know how it was best administered, while Dr. Bennett, who did not pretend to have even prescribed an oxygen inhalation in his life, knew all about it. If he was going to use oxygen, he should use it pure! I thought just as he does, *before* I tried it *pure*; but I soon discovered that it was far more efficacious when combined; and hence in my book I always speak of it as *combined*, and *not* as pure—as *oxidizing* inhalants, and not as *pure oxygen* inhalants.

Do you, or do you not, consider consumption a curable disease?—I think I have already said I do, with certain limitations.

Will you refer to page 102 of Dr. Hunter's book :—"The late Dr. Swett remarks :—' Another important question presents itself. Is consumption a curable disease? and if so, what is the proportion of cases which recover? The general impression in the medical profession is, that a patient with consumption is doomed to death.' " Is there any truth in that general impression, or that that impression exists?—It is much too broadly stated.

It is an extract from Dr. Swett.—Yes.

Do you know that book?—No.

"I have known a number of patients during the last fifteen years who have had the evidences of consumption, and sometimes in an advanced stage, who finally recovered, and are now in the enjoyment of good health." And Dr. Hunter's book goes on to quote :—"Sir James Clark says :—' That pulmonary consumption admits of cure is no longer a matter of doubt.' "—I can say the same to that.

Listen to this. "It has been clearly demonstrated by the researches of Laennec and other modern pathologists." And then :—"Professor Carswell observes :—' Pathological anatomy has perhaps never afforded more conclusive evidence in proof of the curability of a disease than it has in that of tubercular consumption.' And Laennec declares, that the cure of consumption, where the lungs are not completely disorganized, ought not to be looked upon as at all impossible, in reference either to the nature of the disease or of the organ affected. The destruction of a part of the substance of the lungs is by no means necessarily mortal." Speaking of medical science, is not that a comparatively recent opinion that consumption is curable?—I think with Dr. Williams, that we have advanced considerably in the treatment of the disease and the number of cures effected.

My question was simply this—Whether really as regarded the history of this disease it is not quite a recent opinion that consumption is a curable disease?—Well, not very recent.

How long do you say?—Lænnec has been dead I suppose fifty years.

Has he been dead as long as fifty?—Not quite.

But fifty years is not long, is it, in the history of a disease—is it to have arrived at an opinion whether it is curable or not?—As Dr. Williams has told you various diseases were comprehended under the name of consumption. It would be difficult to say how many cases.

I gather from what you said that your attention was drawn to this article before you were called upon to give evidence in this case? Yes.

The Lord Chief Justice. Do you mean the book or the advertisements?—The advertisements and the book.

Mr. Serjeant Parry. Not the article in the *Pall Mall Gazette*?—

I did not see the article in the *Pall Mall Gazette* till some time after it was published—till my attention was called to it as an article which was the subject of the action.

When were you first communicated with with reference to giving your evidence here?—I think somewhere about last December or it may be January.

There were some witnesses examined by Commission were not there?—Yes.

Did you attend?—One of the days I did.

For the purpose of assisting the counsel for the Defendant?—Yes.

This is a point on which I think it right to make a remark. Dr. Risdon Bennett and Dr. Orlando Markham were present at the examination of certain patients of mine before the commission. They were neither subpœnaed as witnesses, nor could they have had any interest in being there, except their zeal in promoting and upholding a libel which had been perpetrated upon a member of their own profession. They were there to suggest to the counsel for the *Pall Mall Gazette*, catch questions, and generally to try to discredit the evidence of the gentlemen under examination. Suddenly the defendants' counsel asked Mr. Meyer, a witness under examination, "*to permit Dr. Bennett to examine his chest.*" As he did not object, in a moment the zealous physician was across the room, stethoscope in hand, ready to examine and determine the nature of a disease treated by me successfully *two years before that time!* Imagine the monstrous nature of the proposition to examine a patient, two years after he had recovered, to determine what his disease *had been!* They must have been reduced to a desperate extremity when they could hit upon no more reputable means of discrediting me than this. This Dr. Risdon Bennett was the very man who lent himself to this discreditable business, and employed himself for days dancing attendance before a commission which had no power to examine him, and had not required his attendance. Did he *for money* engage himself in this business? or was it as a *partizan*? or did he look for his reward in the *injury* he might be able to inflict on a professional brother? He and his employers alone can answer.

Re-examined by MR. KARSLAKE.

I suppose you were asked to attend in the regular way?—I was told there were certain patients to be examined and was asked to attend their examination, and I did when a witness was examined.

And that was one of those gentlemen who declined to have the stethoscope applied.

Mr. Coleridge. If you go into that you must read what was offered.

Mr. Karslake. Were you present?—Yes.

Will you refer to page 4, in Roman numerals. Do you think this fairly represents the views of the profession. “The grand object of their pursuit seems as far off as ever, and they all unite in one confession, that they can recommend neither preventive nor cure for consumption?”—I think that is a very unfair statement.

He thinks it a very unfair statement. Well, perhaps he likes Sir James Clarke’s better:—“*The total inefficacy of all means hitherto adopted for diminishing the frequency, or reducing the mortality of this class of diseases, is of itself sufficient incitement to us to seek for some other method of remedying the evil.*” (Clarke on Consumption, p. xi., preface.)

One question about the chloric acid. Is the chloric acid gas itself very irritating?—Yes.

What are the gases evolved from chloric acid?—I never gave chloric acid and know very little about it as a remedial agent.

If given in the form of vapour of hot water is it irritating or what?—I should think it would be an irritating thing, knowing something of the composition of it. I never gave it and never saw it given.

Now in the passage you referred to which you say indicates oxygen being used, but you cannot follow, at page 127. “I inhale in two ways: warm, from an inhaling instrument; and cold, by filling a chamber with vapour into which I go three or four times a day, remaining from five to ten minutes at a time, according to the strength of the vapour.” And the former gentleman whose letter is to be found at page 127, states this; “Medicines were placed in a glass inhaling instrument, so constructed that the air in its passage to the lungs passed first through a medicated fluid, thus conveying the properties of the medicine in the form of vapour into the lungs. I also inhaled in a small room, which was daily filled with medicated air for that purpose.” And then, at the end of that chapter, it says, “It will be observed that, in all these cases, the oxygen chamber was employed in conjunction with the other inhalations. It must not, however, be understood that pure oxygen was used. Only such addition was made to the air of the chamber as the particular case required. In some cases this was only one-third, while in others one-half or two-thirds. Other medicinal agents were frequently combined for their

local action on the mucous membrane." I think you say that, as far as you know, there is no mode pointed out by which the chamber is to be filled with oxygen, or one-third, or one-half, or two-thirds of oxygen, and medicated vapours introduced?—No.

The Lord Chief Justice. Is it anywhere recommended in the text that this oxygenated vapour should be used?

Mr. Karlake. No, my Lord, that is the only passage I find.

The Lord Chief Justice. That last part seems as if it were a part of the text.

Mr. Karlake. Yes. "It will be observed that in all these cases the oxygen chamber was employed." There is no description of the chamber, but the two letters having been given it is added.

The Lord Chief Justice. That is the only reference to the oxygen chamber.

Mr. Karlake. The only one that I can find.

All this is merely to show what was admitted—viz., that the exact manner of filling the chamber with vapour was not given, and it was not intended to be given, in a popular work of this character. It would have been a folly, if not worse, to have given it, as it could only have resulted in patients attempting to treat themselves, and must have led often to injudicious use and injury.

You have spoken of what you consider to be the views Dr. Hunter entertains, that the oxygen has a direct action upon the tubercle itself, causing the oxidation and absorption of the tubercle?—Yes.

Now, look at the next page and see whether the passage I am going to quote bears that out: "No physician will pretend that cod-liver oil, or any cough mixture, or tonic ever compounded, has power to remove tubercles, purifying the blood, or heal the mucous membrane, even if directly applied." Do you see that passage?—Yes.

Now, look at the next to which I call your attention, at page 114: "The cough is relieved by the expulsion of the mucus,—shortness of breath vanishes with the removal of the local obstructions,—hectic disappears with the subsidence of pulmonary irritation,—and spitting of blood and loss of flesh cease the moment the blood is properly oxydized." There we get the blood oxydized instead of the tubercle. Now turn to the next page: "Combined with the direct application of oxygen to the tubercular deposits and the blood, and with the daily inhalation of medicines to act upon the diseased air tubes and cells, they are all important; for, together they accomplish more certainly that end for which the patient submits his case to the physician—the healing of the lungs." Did you look at those passages?—Yes. The last one is another passage I should refer to with reference to what I said before. That is another passage which seems to assume that the

tubercular matter is oxydized as well as the blood, and of course the tubercular matter must be oxidized in a different way from the blood.

You were asked also whether Dr. Hunter in this work recommends other things besides his inhalations; I think if you look at page 14 you will find that he does in this way:—"But it is frequently asked, 'Do you give no medicines by the stomach?' Certainly, I do—when they are required. If the patient is feeble and losing flesh, I put him upon strong nourishing diet, and give tonics to aid his digestion. There is no objection to use cod-liver oil as nourishment under such circumstances." That follows a passage in page 112. "It is no treatment for a local disease in the lungs to pour cod-liver oil and tonics into the stomach, for they never reach the part affected; and, besides, such medicines possess no properties capable of effecting a cure, if they did. No physician will pretend that cod-liver oil, or any cough mixture, or tonic ever compounded, has power to remove tubercles, purify the blood, or heal the mucous membrane, even if directly applied." Now, let me call your attention to the preface, page 37, in Roman numerals: "I do not hesitate to say that no such results as those detailed have ever before been effected in these diseases. All observation and experience, both in and out of the profession, prove the utter worthlessness of the common routine of fish oil, sedative cough mixtures, and acid tonics administered through the stomach, on which medical men have hitherto entirely relied." You say it is not a fact that they have entirely relied on those, but does that at all events indicate that in this part of the work these things are spoken of as useless?—All observation and experience in and out of the profession do not approve that.

Well, I will correct my text, and say that my knowledge of the results of the *Consumption Hospital*, and of the *experience of patients*, treated there convince me that cure is "*a rare and exceptional event*." If I finish the sentence with his admission of the results of *his own* practice, I am sure he cannot object if I make it general, and assume that *cure is a rare and exceptional event* from that course of treatment which he, Dr. Williams, and Dr. Cotton follow.

Then look at this, at page 39: "Time, stern facts, and the pressure of public opinion, will gradually bring about this desirable medical reform. As young physicians go forth untrammelled in theory and uncommitted in practice, they will, one after another, abandon the treatment by the stomach altogether, and not only adopt, but ultimately carry to even a higher degree of excellence, the rational and successful system I have laboured to introduce."—Well, all I can say is, I think that is a far-off day.

It is to be hoped so, according to your view?—Yes.

I have no doubt he thought the same of bloodletting and mercury twenty years ago. *Then* they were, like cod-liver oil is *now*, in fashion.

You were asked as to the children of consumptive parents. According to your view, is it hereditary?—I think it is in a large number of instances.

Has your experience shown you that?—Yes.

Is that the common opinion of the profession, so far as you know?—Yes.

And largely acted upon by insurance offices and other bodies having an interest in these things?—Yes.

DR. RICHARD PAYNE COTTON, *sworn*.

Examined by Mr. QUAIN.

Are you a Fellow of the Royal College of Physicians?—Yes.

Of London?—Yes.

I believe you are one of the physicians also of the Brompton Hospital?—Yes.

And you have a private practice besides?—Yes.

I will not go over the ground at so much length as before, but ask you generally as to your opinion and experience with regard to the theory of Dr. Hunter of the deposit of carbon on the lung being the cause of tubercle?—I do not believe that carbon is the cause of tubercle.

What is your opinion as to imperfect respiration or insufficient oxidation of the blood being the cause from which tubercle arises?—I do not believe that it is the cause of tubercle, as expressed by Dr. Williams. My view is altogether that which has been already so well expressed by Dr. Williams, and I believe Dr. Williams has expressed the general view of the profession—certainly the opinion taught in all the schools, which I think has been generally accepted, and to that I quite adhere.

Now go on to the passage in page 10, about catarrh, which is called the first step in consumption, and the passage in page 33, in which Dr. Hunter says, “In my last letter I concluded my observations on the diseases of the nose, throat, and bronchial tubes, and now I come to speak of pulmonary consumption, that dread malady in which these affections, when neglected, invariably end.” That is an alarming passage, that diseases of the nose, throat, and bronchial tubes, when neglected, invariably end in consumption?—I do not subscribe to that at all.

Is that consistent with your experience?—It is quite opposed to my experience.

Even Dr. Cotton himself, who did not scruple to appear as

a witness for the defence, and to uphold a libellous attack upon a professional brother, admits quite as much as I have said on this point. "*The frequency,*" says he, "*with which consumptive persons attribute the commencement of their disease to an attack of influenza, or a severe cold, at once points to the influence of these disorders in the development of tubercle*" (p. 92.)

It is true that he says the patients are all wrong in attributing the disease to the colds, that they must have previously had *corrupt blood*, or that their families are of *tainted blood*. But then again he admits that—

"Catarrh also, when severe, and accompanied with constitutional disturbance "and bronchial irritation, appears in many instances to bring into action consumption hitherto dormant" (p. 93).

So that you might be well, or believe yourself so, and might continue so for years, or throughout life, but for the occurrence of an attack of catarrh. You take cold, irritation of the bronchi follows, and then consumption begins. Is it not splitting hairs to say that that cause which *developes* the disease is not the *only* cause? It is all the cause the patient knows anything about, and all that it is worth a straw to him to know about; and no physician writing for his guidance, would think of burdening his mind with worthless theoretical speculations.

While upon that, let me ask you a question about a passage which I did not ask Dr. Williams about, but which appears to be a very alarming passage indeed, in page 53 of the book. Before I read the the passage, let me ask you whether the appearance of blood in the mucous discharge is a thing very much calculated to alarm the patient?—Yes, perhaps beyond anything else; it has a tendency to alarm the patient.

Now, bearing that in mind, let me call your attention to the passage: "Physicians too often cheer their patients by the assurance that the blood only comes from the throat. Let me warn the invalid against being deceived. The throat seldom bleeds—never, unless it be struck by some hard substance and wounded! If blood is coughed up, however small the quantity, it comes from the lungs, and delay is dangerous." Is that true?—I believe it to be untrue.

Does it come from the throat?—Of course there are a very great number of cases in which blood is coughed up where it does come from the throat, but I mean it does not necessarily come from the lungs.

So that if there is a streak in the mucus it is dangerous—because

the words are—"If blood is coughed up, however small the quantity, it comes from the lungs, and delay is dangerous?"—I should not infer from that alone that a person was consumptive.

Does it come from a man except on being wounded?—Certainly, constantly.

Then it goes on: "Fortunately, it generally occurs before the disease is far advanced, and if proper treatment be employed it can generally be arrested. The great error committed by physicians is in treating the symptom itself, while the cause which produces it is left in undisturbed possession of the lungs." Then it goes on: "It will be asked if hæmorrhage never occurs where the lungs are not diseased? Yes, there are three cause which may produce it. First, injury to the lungs, as from a violent blow; second, organic disease of the heart, and third, in females it may occur every four weeks instead of the usual periodic excretion. But if a person spits blood in whom none of these causes exist set it down as a sure sign that the lungs are unsound?"—In answer to that I should consider the assertion unsound.

He considers my opinion unsound, and the object he had in saying so, was to create the impression that I had for some reason made more of this symptom than it merited.

Dr. Cotton himself admits that out of 582 patients at the Consumption Hospital, it occurred in 326 of the cases (p. 121.)

Professor Swett says—

"So rare is spitting of blood in any other disease of the chest that it points most unequivocally to the true nature of the case (p. 245). Whenever I am called to a patient who has had spitting of blood, I always mark him as probably a tuberculous case."

Sir Thomas Watson uses almost the same words as I have employed—

"*Hæmoptysis* is a kind of expectoration—the expectoration of *blood*. I have already spoken of this symptom as connected with phthisis, and have stated my belief on that subject; viz., that if a person spits blood who has received no injury to the chest, in whom the uterine functions are healthy and right, and who has no disease of the heart, the odds that there are tubercles in the lungs of that person, are fearfully high. Excluding cases of amenorrhœa, and of mechanical injury to the thorax, Louis did not meet with a single example of hæmoptysis among twelve hundred patients, except in such as were phthisical."—*Watson's Lectures*, p. 187.

So much for the wretched attempt to make capital out of that point. If Dr. Cotton did not know that others held precisely the opinion expressed by me, he must know very little of the literature of his profession; and if he *did*, what does it mean

that he censures me for saying what Sir Thomas Watson has said in almost the same words?

The Lord Chief Justice. If you get a congested state of the tonsils, which very often happens, does not it?—Very frequently indeed.

That is no evidence of consumption?—I have known over and over again where a small amount of blood is expectorated to pass off and not be succeeded by anything serious.

I am asking you whether you would not call this an alarming doctrine?—I call it an exceedingly alarming doctrine. I do not know whether it is competent to me to mention a case that occurred to me of a gentleman who had been to Dr. Hunter.

We cannot go into that.

The Lord Chief Justice. Though I take it that he may have overstated this in saying that it is sure sign that the lungs are unsound, still spitting of blood is very serious?—Yes, it is serious and is always a symptom to be looked to. I am not pooh poohing that it is a serious matter, but I am merely saying that, because a person spits blood he must necessarily be consumptive, which I understand those words to imply.

Mr. Quain. Now, a question about inhalation at the Consumption Hospital, and your private practice. Have you tried inhalation very extensively yourself?—Yes, I believe I may say very extensively.

When did you first go to the Consumption Hospital?—I have been connected with the Consumption Hospital 18 years. I have been assistant-physician, I think, 8 years, and full physician 10 years.

During the whole of that time, have you tried inhalations?—During the whole of that time, I may say safely, I have been in the constant habit of employing inhalations with both the in and the out patients, and at the commencement of my career at the hospital, Dr. Snow, who is well known, but he is now dead, and who advanced the inhalation of chloroform he and I carried on a number of experiments in inhalation at the Hospital for Consumption.

Dr. Snow, I think, occupied himself chiefly in the mode of administering chloroform?—In administering chloroform—he and I together, I mean; he assisted me in those experiments.

Did you try a great variety of substances?—I think I tried every possible substance which could be administered—I have at different times.

Therefore you had large experience?—I have had just the experience the Hospital has afforded me.

Have you tried all those substances, with the exception of chloric acid which were mentioned yesterday. Conium, stramonium, iodine, and others?—All those. I may mention that the pharmacopœia put in yesterday or the day before contains three or four prescriptions there, but those do not represent the amount of inhalation which we use there; it was only a copy of it for the convenience of our dispenser; we employ a great many more.

The actual inhalants you use are very many more?—Yes; I think we have only four or five there.

There are seven?—I think we employ more than that. Some of the physicians who are my colleagues, and I myself, have employed a great many more.

It may be that Dr. Cotton and the other physicians have a "*great many more*" prescriptions than those which have been published and concerning which the professional world knows nothing. Why, if they were of any value, were they not published? How does it happen that the respectable chemist who got out all the hospital prescriptions should have left "*a great many more*," unless they were regarded as worthless? And it is also strange that they should happen to be just those which are *said* to contain some of the *very* ingredients used by me, but concerning which unfortunately there are no records in the hospital prescription book! They *may* exist; I hope they do.

Have you used a great variety of inhaling instruments?—Yes; a great variety.

The Lord Chief Justice. You have never used inhalation of oxygen?—Yes; I have.

At Brompton Hospital?—No, not Brompton, but private practice; that is to say, I can scarcely say that I used it, for they happened to be patients using it at the time I was called in; so I have had some opportunity of witnessing its effects, but that is limited; I only know three of those cases.

Was that inhaling free or pure oxygen?—Not free oxygen; it was what was introduced lately by Dr. Rigaud.

Quite recently?—Yes; six or eight months ago.

That is chloride of lime, is not it?—No; it is some powder.

Do you mean bleaching powder?—I do not know what. I might mention that I have often used in inhalation the very thing which Dr. Hunter, I think, in his evidence described he used in the form of pastille. I have not used it in the form of pastille, but what comes to the same thing: I have used the ingredient; the mere fact of its being in the form of pastille is for convenience. I have used a compound of nitrate of potash and stramonium several times.

Here we find a man, who does not think oxygen would be likely to do good, has been within six months, aiding and abetting in its administration! Was he trying experiments on the three unfortunate invalids? Why, if Dr. Williams had tried the

whole thing years ago, did he not rest satisfied with Dr. Williams's experience? Perhaps he was, like many others, not aware that he had ever made any, until he heard his evidence in the Court. As he evidently knows nothing about oxygen, and does not pretend that he ever saw it given in more than three cases, and those six months after this action was laid, all he can know about it is not worth refuting.

But there is another point to which I think it well to direct attention. Here he is again experimenting with some compounds, the nature of which he does not pretend to know, and aiding and abetting the treatment of three unfortunate patients by some mixture which he assumes to be oxygen. When asked what it was, all he knew about it was that it was "*some powder!*" Is not this *very* scientific?

Then what vast experience he must have had with stramonium and nitrate of potash. He has been perhaps twenty-five years in practice, and he tells us he has used them "*several*" times. I wonder whether the several times were not within the past few months. Since he discovered the fact that I used these ingredients extensively in making the room vapour, I am afraid he has been stealing my ideas and trying experiments *several times*—very recently. Why don't he stick to cod-liver oil?

After what your experience has been of inhalations in pulmonary disease, and from the various trials you have made of it for many years, I will ask you whether you have seen quite recently in the manner pointed out by Dr. Bennett, I believe, the administration of oxygen?—Yes.

You have had only three cases in your own hands?—Only three cases.

Tell my lord and the jury what you observed about it?—I did not observe any satisfactory result. The idea which I have of consumption, and the idea which, I think, is entertained by most of us on consumption—I am speaking now of purely consumptive disease, not the class of cases which we had before us yesterday of Dr. Hunter's, which were not cases of consumption, as it appeared to me, but which were cases of pure asthma, obstruction of the nasal passage, and so on, I am not speaking of those, because I think if you can give them a little oxygen it is likely to be a useful adjunct—lead me to think that the inhalation of oxygen is really undesirable in such cases.

The Lord Chief Justice. In cases of pure consumption?—Pure tubercular consumption. If a patient was afflicted with consumption it is the last inhalant I should employ.

Just give your reason?—Because in consumption we have the evidence before us of excessive wear and tear of structure.

Do you mean of the lungs?—Of the whole system, the lungs, of course, included. Then we have in addition, as noticed lately, and of which I have made some observations, an increased temperature of the body; it is one of the earliest symptoms of consumption, that the actual temperature of the body is increased, showing that such increase is part of the morbid process.

Mr. Quain. There is a thermometer for ascertaining that bodily temperature, is there not?—Yes; there is a thermometer for the purpose; and I should hold it to be the most likely result of inhaling oxygen that you would be adding, if I may use the expression, fuel to the fire, or perhaps reversing it by increasing that wear and tear.

The Lord Chief Justice. What do you mean by the expression, wear and tear?—The decomposition of the animal tissues.

Wear and tear. Does not that proceed from the inability of the system to supply fresh matter?—What I meant was the natural wear and tear going on in all of us.

But in consumption is there an increased degree of this decomposition?—Yes; I think so, as evinced by emaciation.

Would not that follow?—Unless you had reproduction of the tissue worn out, there would be emaciation.

Is not it rather that the system is unable to assimilate new matter?—That is another reason, of course, for the emaciation.

In addition to that there is more rapid decomposition?—Yes. I think the respirations at the nose are more accelerated.

Would not that be caused by an insufficient supply of oxygen?—No. You will find that the respiration is very early accelerated in consumption. There is another reason why we should not give oxygen, because we know that it is an excitant; and animals which have been made to breathe oxygen, labour obviously under excitement, and such excitement would be likely to produce or aggravate any tendency to spitting of blood. I merely give that as my opinion, as the reason why I should not do it myself.

It would cause that by stimulating the lungs?—By stimulating the surface of the lungs. That I give as my opinion.

Mr. Quain. And that would be likely to produce hæmorrhage?—It would be likely to produce hæmorrhage. It did so, I think, in one case in point, where a gentleman who came to me was inhaling this, and he already had a very violent spitting of blood. Of course, one case is not sufficient to prove, but it is sufficient to warn.

He thinks the “respirations at the nose (!)” are accelerated. I should rather imagine the respirations of the lungs *were* accelerated, and that this was one of the indications of want of oxygen, or want of air.

Sir Thomas Watson, speaking of this, says :—

“Now, upon what does this symptom depend? It may ultimately be referred “to an altered proportion between the quantity of atmospheric air that reaches the lungs and the quantity of blood that is sent into them from the right side of the heart, to be converted from venous to arterial. That, doubtless, is at the “bottom of almost every case of dyspnœa.”—(vol. ii. p. 3.)

So that the quantity of air being lessened by the disease, the patient breathes quick, because he wants more oxygen than he can get from the lessened volume. He must make up in *frequency* what he lacks in *quantity*. Oxygen diminishes this excitement by supplying the want which created it, just as the free admission of air to the lungs is followed in asthma by immediate relief of the distress, and by diminution in the number of the “*respirations at the nose!*”

He evidently is afraid the sick may prefer oxygen to his oily compounds, and so he tries hard to frighten them into the idea that it will increase “the decomposition of the animal tissues.” If it will do that why may it not “decompose” the *fatty tubercles*, for which it has a much stronger affinity than it has for the “animal tissues.” Then, too, he is afraid of hæmorrhage. It might bring on that, he thinks. I wonder what it was that produced hæmorrhage from the lungs in 326 cases out of 586 under his care at the Consumption Hospital? They don’t use oxygen down there! Could *this* have been caused by *cod liver oil*, I wonder?

You have heard, so far as the evidence goes, that the only way of giving oxygen was by means of chloric acid, mixed with these various ingredients, and to put a spoonful at a time of the inhalant into the hot water. Do you think that oxygen administered in that way for the purpose of oxygenating either the blood or the tubercle could reach it?—I do not understand what substance would be used.

We had the description yesterday :—6 drams of chloric acid; $1\frac{1}{2}$ drams of iodine; $1\frac{1}{2}$ drams chloroform; and the rest stramonium.—That I do not know, because I have not had any experience in chloric acid, but chlorate of potash and nitrate of potash, which is another way, as I understand, of diffusing oxygen.

Give us your experience of the elimination of chlorate of potash, or nitrate of potash?—I have great doubt myself; it is a chemical point, of course, and I would defer that to the chemical evidence; but I have great doubt whether nitrate of potash, when burnt, as we burn it, does

give out pure oxygen. I am doubtful, but I have often made the experiment.

Why did he not get Dr. Odling, the chemist, to remove his doubts? It certainly was not very scientific to be administering to sick people a compound the nature of which he did not understand. This speaks as badly for his conscience as his chemistry.

Now, as to the inhalation of the various other inhalants which you have described. Give us generally your experience of the effect of them in consumptive cases.—I look upon them as auxiliaries to treatment, and, in some cases, useful auxiliaries to treatment, but not as exclusive means. I should be sorry to trust alone to inhalation, and have been generally disappointed in the general results of the treatment by inhalation.

Every man may be supposed to know most about his own practice. I think it very likely that his patients were quite as much *disappointed* as himself. But surely Dr. Cotton would not wish to instance his own bad success as proof of another physician's want of skill. Considering his experiments with compounds which he did not understand, the fact of his failure ought to excite no surprise. It would have surprised me had it been otherwise. Haphazard practice seldom succeeds.

You mean as far as its curative power in pulmonary consumption is concerned?—I am convinced that it has no curative power; at least, I have seen no evidence of it. I may say, that we do not use it with a curative object. There are many things which we use with a curative, not merely a palliative object; but I am bound to say, I do not think there is anything really curative. I have not seen anything which would justify my opinion that it was.

The difference between Dr. Hunter's view and yours is, that while you would use this inhalation as merely auxiliary to the ordinary remedies, he treats the inhalation as the curative treatment, and the administration of ordinary medicines as auxiliary?—From the evidence of the patients which I heard yesterday, I understood that Dr. Hunter had treated them in precisely the same way as we should have done.

As regards the advertisements and the book of Dr. Hunter, the inhaling process is the main thing, and the ordinary medicines administered through the stomach are subsidiary to that main principle?—I gather that to be so.

Whereas your system is exactly the reverse?—It is.

You treat inhalants as merely auxiliary?—As merely auxiliary.

And not as substantive means of cure?—Just so.

Mr. Karlake. What Dr. Cotton said was, that he had used them with a curative effect, but in the result it had not a curative effect?—What I meant was, that we have used agents which, if active at all, would or might be curative in their result, but they were palliatives, soothing things merely, and specific things.

What does he mean by “*specific things*?” Some catholicon? And yet that cannot be, for he says they “*might be curative*,” but they won’t. “*Specific things*” probably means things which might have been specific, but weren’t. I hope the reader understands it, for I am sure I do not.

The Lord Chief Justice. And they have failed?—According to my observation they have failed.

Mr. Quain. But you have used largely those four classes of drugs mentioned at page 113—sedatives, astringents, alteratives, and expectorants?—Do you mean as inhalants?

As inhalants.—I think Dr. Hunter there is not speaking of inhalants. The medicines which it is necessary for the patient to inhale are of four kinds:—First, expectorants, to expel the mucus; second, sedatives, to allay irritation; third, astringents, to diminish secretion; and fourth, alteratives, to change the action of the diseased membrane.

The Lord Chief Justice. What would you administer in the shape of alteratives?—Well, it is very difficult, I think, to apply those four words to the things.

Mr. Quain. What could you inhale as an alterative?—I have the same difficulty in answering that question which I think Dr. Bennett had.

It is an alterative to change the action of the diseased membrane?—I should not give that term. I do not know what Dr. Hunter means.

The Lord Chief Justice. What is the general medical use of the term?—We should not use it medically—an alterative expectorant.

It is as distinguished from expectorant.

Mr. Quain. It is an inhaled alterative?—I should not know what was meant by an inhaled alterative.

The Lord Chief Justice. Could that apply to an inhalant?—I should think not. We do not apply the term alterative to a thing that is inhaled. It is simply a thing which explains itself, which, in some way or other, alters the existing state of things. It is difficult to explain.

But when he tells you that he uses an alterative inhalant, do you consider anything by it?—I do not consider anything by it.

Does it convey anything to your mind?—No; it does not convey anything to my mind.

Mr. Quain. Although you are acquainted with the alteratives?—Although I am acquainted with alteratives. If I were asked to prescribe an alterative inhalant I should not know what to give simply by those terms.

If he knew what he wished to *alter*, could he not exercise his judgment? We talk of alteratives by the stomach, and each physician exercises his own judgment which will be best in any particular case. No physician could decide until he understood all the circumstances of the case. As I was not writing a pocket guide to enable Doctor Cotton and his friends to apply *my* practice to *their* patients, I did not think it necessary to enter into these details.

Of course, you have tried very extensively cod-liver oil?—Yes; I have.

Which we are told by this book, as far as acting on the disease is concerned, is quite useless, while you will find at page 114 that there is no objection to the use of cod-liver oil as nourishment under such circumstances; therefore, Dr. Hunter would not use it as a means of cure. What is your opinion about it, as a means of cure?—I consider it a most important adjunct in the treatment. I should not treat consumption with cod-liver oil alone, but I should not think that all had been done that ought to be done for the patient unless cod-liver oil was tried.

The Lord Chief Justice. Have you found patients improve when they have had cod-liver oil under your direction?—Oh, yes, my Lord; as a general rule, they improve. Of course, not all of them.

Mr. Quain. In conclusion, I will put one general question. Having taken the book, and read it as a whole, as an educated medical man well acquainted with this subject, is there sufficient in that book to guide you, if you wished to adopt Dr. Hunter's system?—No; there is nothing to guide me as to his treatment.

As to the general practice of the profession, whenever anything is proposed as a new mode of treatment, or cure, is it the general invariable usage for the medical man who proposes it, to give minutely what the cure is, and instructions to his medical brethren how to adopt it?—In answer to that, I may say that I do not know of a single book written by a legitimate practitioner of medicine—a book which has for its object the introduction of a new system, or a new remedy—which does not go minutely into the explanation of that remedy, what it is, and how it is employed, in order that he may give another medical practitioner the opportunity of employing it, and does not let it rest on his own dictum.

Is it not usual to give the formulæ?—Yes.

And as they vary from day to day?—Yes; in a greater or less de-

gree. The medical profession naturally hates anything which is kept a secret. I mean anything that is beneficial to the public should be openly avowed, so that all may benefit by it.

Medical men are compelled to make known the results of their practice for the benefit of their brethren by *courtesy* but not by *right*. But then this courtesy implies reciprocity. The British profession refuse to reciprocate. They not only libel colonial physicians when they come to England, but keep them from registration or membership of Medical Societies. If we gave them the fruits of our experience we should have nothing except foul words for a recompense. This may suit their idea of comity, but it does not suit mine. Still I had no idea I was open to the charge of keeping any secrets from the profession. I wrote for the public; and the public would only be injured by prescriptions which they could not know how to apply. Had I known how deeply solicitous my brethren were to get at my prescriptions, I might have written them a pocket-companion to prescribe from, embodying my experiences for the past fifteen years. But we never know these things until too late. If they will form themselves into a class, I will give a course of gratuitous lectures, and illustrate them by clinical cases. What more can I offer to prove my fraternal benevolence. I really never should have thought of complaining, because Dr. Cotton did not give his prescriptions; for, after I had read his book, I felt no curiosity to inquire further into the results of his practice.

But is not Dr. Cotton equally *sinning*. The profession get the little book professing to contain the prescription of the Consumption Hospital, and naturally think they have got all. No, says Dr. Cotton, we have kept some back, we have a great many more which we do not make known. Are *they* kept *secret*? But after all, looking at the results of the hospital practice, few will think it worth while to hunt up the missing formulæ. I will do Dr. Cotton the justice to say, that I do not believe he kept them back by *design*. I presume they were *all* submitted to the respectable chemist who compiled the little book, and that he left out those which he did not think worth putting in. This will, I hope, relieve the Doctor's mind from any further anxiety on that score.

Now, I will ask you as to Dr. Hunter's way of giving a case,—the case of Mrs. Johnson, of 5, Cornwall-terrace, Colney Hatch, page 18 in the English cases. She was “twenty-eight years of age, of slight frame and feeble constitution : has been in delicate health for nearly ten years, her ailment having commenced with catarrh which became chronic and was followed by coughing. Eighteen months ago her suspicions were first awakened with regard to the state of her lungs. At the time of her examination she had a constant hacking cough in the morning, attended by the expectoration of a grey and yellow matter, and she had hurried breathing, a sense of tightness across the chest, and occasional pains across the left side of the chest. She suffered from frequent cold chills, and slight feverishness daily, with occasional night sweating. She was much emaciated and her strength was greatly impaired. There was a granular and ulcerated condition of the throat and tonsils. The pulse was quick and feeble; the digestive functions were impaired.” Then comes the treatment to which I call your attention :—“She was immediately placed under active treatment by inhalation, with tonic alterative medicines internally, and appropriate applications to the nose and throat.” Now, is that the way, if you wanted to state a case for the information of the medical world and the benefit of the world in general, in which you or any other medical man would describe the treatment ?—Certainly not.

Take, again, Case 5, which follows the one I have just read : “Miss H——, a lady of highly nervous temperament, of a consumptive family, had been suffering since July, 1863, from general debility and irritating cough. October 21st, 1864, she became a patient. There was great emaciation, languor, and debility; her pulse was 100, and feeble; there was constant cough (which sometimes occurred in severe paroxysms), with copious thick expectoration of a greenish colour, great shortness of breathing, hectic with night sweating, and there had been occasional attacks of blood-spitting. Examination of the chest revealed the existence of tubercular deposition, with extensive bronchitis. It was, in fact, a case of chronic consumption. She was put under treatment by inhalation, with appropriate constitutional remedies.” That is the only indication you have of the treatment ?—I never saw a case so described in any book which is generally received by the profession.

Take, again, Case 7.

The Lord Chief Justice. That is enough; it is all matter of observation after all. All that these cases show is, that Dr. Hunter's treatment was adopted, and it was found successful, without any detail as to what the patients received.

Mr. Quain. Quite so; it is appropriate remedies and inhalation merely.

These cases were *not* published for the information of the *profession*, but for the instruction of patients and the public.

They were merely intended to show the results attained by carrying out the plan of treatment indicated in the book. They contain all that such people care to know, and they give name and address, so that they may understand them to be *bonâ fide*, not *sham* cases such as are too often published. Dr. Cotton has not published *any* cases, therefore we neither have the *fruits* of his skill, nor his prescriptions. Perhaps if he could publish a few good cases he would feel more kindly towards those who *are* able to do so.

Cross-examined by Mr. COLERIDGE.

I suppose you have given us your opinion on these matters? I suppose the mind of the profession has been in a state of flux on this subject for a good while, has it not?—Upon what subject?

Upon the causes of consumption?—In a state of flux I understood you to say?

I mean, you are unsettled about it—making experiments which are going on largely?—Certainly.

And what is true one year is not true another?—Yes, we are gradually learning.

And very slowly, are not you?—I am afraid we have not made much progress here.

Have you written a book on the subject yourself?—Yes, I have.

Where should I find the prescriptions in this book?—Well, I cannot tell you; of course, if you will send it to me, I will point them out to you. I recollect there was a prescription at the bottom of one of the pages.

I daresay there are. I should like to see your prescriptions (handing the book to the witness).

The Lord Chief Justice. What is the book upon?

Mr. Coleridge. Upon the nature and treatment of consumption, my lord.

The Witness. I could point out several, but here is one at the bottom of page 122, in which you will find (I will turn it down) that I am speaking of counter irritants as applied to the external treatment of consumption; and I may mention that at the hospital we use a certain proportion of iodine, and I can give you the formula of it here—that is to say, so much iodine.

And how do you provide for the variation of the prescription from day to day?—My book does not contain cases like Dr. Hunter's.

It does not contain cases at all?—No; it is a general thing. It is not usual to fill a book with cases; we give general results.

Do you mean to say a great many men do not collect cases in books?—They do sometimes, but it is not necessary.

You put upon me that it is not usual?—It varies.

It is nothing unordinary, is it, to put cases?—If people put cases I should say they do not vaguely state “my experience,” and so on.

First of all, you put upon me that books like yours contain no cases. Now do you mean to represent that, in the works of men of high character, there are not collections of cases—works of high character on this very subject?—Many books contain collections of cases, no doubt. All I say is, that when such books do contain cases—of course, you may find exceptions—but the rule is, that the treatment is given more or less specifically; but with regard to a more correct answer in speaking of the inhalations that I use, I specify each one—not as Dr. Hunter has done there, merely “inhalations,” I give every single thing, mentioning it, and how it is to be given; and the prescription to which I refer is at page 223.

You say there are no cases in your book; and I shall not therefore find prescriptions from day to day?—No, it does not contain cases.

You rather surprise me. You say that an alterative is that which alters the condition of things; do not stimulants and expectorants also alter the condition of things?—Of course they do.

What is the difference, then?—The word alterative is very difficult to define.

It is a common name?—It is a common term.

Regular medical men make use of the word alterative?—Yes, but we do not use the word alterative as applied to inhalation.

The Lord Chief Justice. I have often heard the term. Does it mean anything that acts upon the system generally?—Yes, that acts upon the system generally.

That alters the system?—I take it to act in different ways. It is difficult to explain how it acts, but it is a thing which produces an alteration in the system.

Is it applied to something which produces a change locally?—No, I never heard it applied in that way.

Mr. Coleridge. May not you give various drugs which are applied for specific purposes in various forms that they become alteratives?—We use alterative only for a medicine taken internally.

But are there not medicines directed to some particular organ, which given in combination with others in a particular way, as prescribed by physicians, to have a specific action on a particular organ, act as alteratives?—Oh, certainly.

The Lord Chief Justice. Are they intended to act through the system?—Through the system.

Does the term alterative necessarily imply something which, though intended to act upon a particular part, acts by its operation on the general system?—Yes; which acts on the general system.

Mr. Coleridge. Does not that depend on how it is given? May you not give, for instance, calomel as an alterative?—Oh, yes.

But it is generally given to act upon the liver?—For a variety of objects it may be given.

It may be given for the purpose of acting on the liver?—Yes.

And may be given also as an alterative?—Yes.

That is an instance of what I mean—that it depends upon how it is given, and on what combination, and under what circumstances?—Yes.

That is all you can tell us about alteratives. You pledge yourself against this theory about carbon. Have you any accurate information that you will pledge yourself to as to the source of tubercular disease?—No, I cannot refer it to one particular thing.

Is this true, that “The term phthisis is now universally and exclusively applied to a peculiar and obscure condition of the whole system, in which the healthy materials required for the growth and reparation of the body are imperfectly supplied by the process of nutrition; whilst the elements of a morbid substance, finding their way into the blood, are deposited as tubercle in certain internal organs, chiefly the lungs; ultimately causing their destruction, and death. This state of system is precisely identical with that known by every one as struma, or scrofula. Phthisis and scrofula are in fact merely varieties in development of the same malady; the difference between them depending upon the tubercular substance being in the one case principally deposited in certain internal and highly vital organs; but in the other, more generally in glandular and superficial structures, the soundness of which is far less essential to the functions of life?—Well, I recognise that as a portion of my own book, and I believe it to be true.

The Lord Chief Justice. Will you give me the reference to that book?

Mr. Coleridge. It is page 2 on the “Theory of Consumption,” edition 1858.

“Phthisis may be either inherited or acquired, but of its intimate nature our knowledge is very limited. The same remark, however, is applicable, and perhaps with equal truth, to the other affections also. In consumption, as in many other maladies, we are permitted to recognise the disease only in its effects. For example, we can examine tubercle and ascertain its chemical and microscopical character as easy as we can the lithate of soda, or diabetic sugar, but of the causes from which these may spring—which are, in fact, the disease themselves—we are completely ignorant. It is evident that in all such cases there must be something which constitutes the malady; but it would be vain to search after it—it has no individuality; it is a process which, like many others, is so subtle and far removed, even from our conceptions, that it seems destined to remain for ever beyond our reach; we are allowed to do nothing more than study its laws, and in some measure regulate its actions.” Perhaps you recognise that as a passage in your own book?—Yes.

You agree to it?—Yes.

“It appears that in the year 1840 the mortality from all diseases collectively amounted in England and Wales to 413,681, of which

number 71,106 were attributed to phthisis, showing that more than one-sixth and not much less than one-fifth of the deaths in that year proceeded from this disease. It has been computed from these records that one person in about every 310 of the entire population annually falls a victim of phthisis. With such facts before us, it may certainly be said that the sword and the famine are in their effects comparatively insignificant to consumption, and that there is no other pestilence which has vanquished half so many."—Those figures of course apply to when that book was published, I think about five or six years back.

1858 is the date?—Those figures are quite accurate—they were given me by the Registrar-General of that year.

You gave your theory about it; but you do not affect, I suppose to speak with anything like certainty?—As to the cause do you mean?

Yes.—I cannot define any one particular cause for consumption. It is the general result of a vast number of causes.

You say you are completely ignorant of that which is the disease itself?—I am not ignorant as to tubercle.

You say, "We can examine tubercle and ascertain its chemical and microscopical character as easy as we can the lithate of soda or diabetic sugar; but of the causes from which these spring which are in fact the disease themselves we are completely ignorant?"—Yes, it is so.

Of course you can examine any physical substances by the microscope?—Yes; what I mean is that we do not know precisely.

You do not pretend to give a confident opinion as to how tubercle grows?—We know how it grows—We know how it is deposited.

How it is first created?—We know how it is first created.

Then I put it to you in your own words—do you know of the causes?—By the causes I mean constitutional causes—why one person should be tubercular and another not; we cannot point out that particular difference which makes one tubercular, and another not.

There are a great many cases, are there not of tubercular deposit in which, as far as you know, there is nothing hereditary whatever. I will not ask whether the majority, but at all events, a great number?—I have traced them, and you will find statistics in that book; I have traced to hereditary causes more than a half—there are a large number of acquired cases; but I recognise those that are hereditary as very important and in that I may mention that I decidedly differ from Dr. Hunter who states that they are generally acquired.

At all events you differ from him as to what he says with regard to inherited and acquired disease?—Yes.

It may be well, since this question of *inheritance* is one about which most persons feel some concern, to say, that in the opinion of the most reliable authorities, but a small proportion of the cases can be traced to inheritance, many

physicians dispute its reality, and others give it a very limited influence in accounting for the disease. Professor Swett is an advocate for it to a considerable extent, while Dr. Walsh, one of the consulting physicians to the Brompton Hospital, regards it more lightly. In my book I admit inheritance to a limited extent, and I admit inherited predisposition to a still greater extent. But still we find at least three cases out of every four to be accounted for, and I hence place the principal stress upon those influences which I believe directly or indirectly tend to set up the disease in persons or in any predisposed to it. This is all the difference there is between my views and those of Dr. Williams. His *inflammatory* ideas go even further than mine. But after all it is mere opinion, and when these medical men come forward and say, that they do not believe my view to be correct, they know that I could easily have put twenty medical men in the witness box willing to say they did not believe theirs. No honourable physician, knowing these differences to exist on every material question of causation, would seek to injure his professional brother by such a course. It is this which justifies me in regarding the whole thing as a miserable conspiracy to inflict injury upon me. If they wish to dispute my theories, why do they not do so through some channel open to me for reply.

Now did I understand you to say that you thought he had overstated the constancy of the relation of the spitting of blood to consumption?—May I be allowed to ask the page of Dr. Hunter's book.

My friend asked you some questions about it and you said that you thought he had greatly overstated it. Now do not understand me as suggesting to you that there are not other causes for it, because, unhappily there are; but is not spitting of blood commonly the indication of consumption?—Yes.

Very common, is it not?—I hold it very different from spitting blood as indicating consumption.

I have no doubt you can explain the distinction.

The Lord Chief Justice. The passage is this: "Physicians too often cheer their patients by the assurance that the blood only comes from the throat. Let me warn the invalid from being deceived. The throat seldom bleeds—never, unless it be struck by some hard substance and wounded. If blood is coughed up, however small the quantity, it comes from the lungs and delay is dangerous."

Mr. Karlake. Then there is the next paragraph: "It will be asked if hæmorrhage never occurs where the lungs are not diseased?"

The Lord Chief Justice. Yes; and he gives three instances: "First, injury to the lungs as from a violent blow; second, organic disease of the heart; and third, in females it may occur every four weeks instead of the usual periodic excretion."

Mr. Karlake. "But if a person," it goes on, "spits blood, in whom none of these causes exist, set it down as a sure sign that the lungs are unsound." That is the passage we quarrel with.

Mr. Coleridge. Now, attend to this passage: "Without attempting to cast a shadow of doubt on the admitted fact, that hæmoptysis is much the most frequently a consequence of tubercular aggregation, and, as such, symptomatic of phthisis rather than tuberculosis, I am satisfied that it is also not unfrequently a symptom of the general disease, and the primary existing cause of its localization—that, in fact, the ancient doctrine is true in many cases. To strengthen this view, there is no occasion to refer to the individual opinion of the older writers, as we find it admitted by Laennec, Andral, Louis, and Fournet. Andral enumerates among the alterations which precede the development of tubercles—an effusion of blood into the tissue of the lung. It has occurred to me more than once to have the professional charge of young persons inheriting an unquestionable tuberculous constitution, in whom, without one physical sign of the existence of tubercles—not even the prolonged expiration in any part of the lung—hæmoptysis has suddenly set in, and been followed by the deposition of tubercles and all its consequences. Illustrative cases are recorded by Andral, where the effused blood became the nidus of the morbid deposit." And so he goes on at considerable length. That is from Mr. Ancell's book. Do you agree with that?—It is difficult to give an answer to so long a question without more study of it.

The substance of it is, that the effusion of blood into the tissue of the lung precedes tubercle, and is the cause of its localization?—I believe it precedes tubercle, but I do not think it is the cause of its localization. I imagine that that is an old book.

Well, the date is 1852. It is six years before your own: that antiquates it possibly; but Mr. Ancell is a man who understands something about it. Is he not?—He is not alive now.

But he was a man who knew about it?—Yes; I might mention, with great respect, to the memory of Mr. Ancell, who was a personal friend of mine, and who was generally esteemed that he had a general practice, and had not so great an amount of experience of consumption as we, who are attached to hospitals, have.

Do you mean that he wrote treatise on tuberculosis, the constitutional origin of consumption and scrofula, without having much experience?—I mean that such a large book as that—I know the book very well, and respect the book, but many of these books must be taken from other books; he could not speak from his own experience.

We are all liable to error?—No doubt; we are all liable to error.

You generally approve of Mr. Ancell?—Yes; but not in that view he takes there.

I will not go through the whole of these matters of opinion with you: but when were you first consulted about this matter?—Speaking from memory, I should think it was a month or two, or longer; perhaps three months previous to the time fixed for the trial.

Had you seen the book before the article was written?—Do you mean Dr. Hunter's book?

Yes?—No.

I do not in the least impute it to you, but I wish to have it as a fact. You were not consulted before the article was written?—No.

You were not consulted, and it was not written on your authority at all?—No.

You never saw it?—No; for months after it was published I did not even know of its existence.

You were consulted then many months afterwards?—Many months afterwards; but I do not know how long.

The article appeared in November 1865?—Well, it must have been somewhere in January or February.

There is one question I want to ask you. Do I understand you to say, that you do not believe that when you saturate paper with nitrate of potash, and burn it, that oxygen is evolved?—I believe that under correction, because of course it is a chemical question.

But is that your belief?—It is a question I cannot speak very positively about; but my belief is, from the odour and so on, that a nitrous oxide is given off, and not pure oxygen.

But is a vapour or gas given off, by which you have many of the specific properties of oxygen?—I take it that it would have a different effect from pure oxygen. The product of the combustion of nitrate of potash I consider to be different from the inhalation of pure oxygen.

I do not mean to say that at all. I know it is different; but I want to know whether you really mean to doubt that by saturating paper in nitrate of potash and burning it, you get a quantity of oxygen evolved?—I doubt it.

The Lord Chief Justice. Pure oxygen?—Pure oxygen, we get some of the compounds.

What you want to get into the lungs is pure oxygen?—Yes; and I do not think we could by that means.

Mr. Coleridge. Supposing you introduced into the lungs that which has the same, or many of the specific qualities as oxygen, is not that the same thing?—Perhaps you will repeat the question.

When you introduce this, what do you call it?—Nitrous oxide, I suppose; I do not say it is; but I merely give my opinion.

The Lord Chief Justice. Assume it to be.

Mr. Coleridge. Assuming that, would not it have the same, or many of the properties—purifying the blood and many other matters for

which oxygen is used, as oxygen itself?—I should say it would not have the same effect.

None of them?—It is difficult to say, but I should not expect the same effect as from the inhalation of oxygen gas.

Dr. Cotton conveyed to the Court the impression that the product of nitrate of potash in combustion was not oxygen. Mark well, too, that he distinctly said it was entirely different from oxygen. Whether that evidence was honest, or given intentionally to mislead, I leave the reader to determine, after I have stated what nitrous oxide really is. The air is composed, in round numbers, of one part of oxygen and four parts of nitrogen. It depends on the one-fifth of oxygen which it contains for its action on the blood—nitrous oxide is the second compound of oxygen and nitrogen, and is composed of one equivalent of oxygen and one of nitrogen. It also depends on its *oxygen* for its action on the blood; and, as it contains a very much larger proportion of oxygen than the air, it is given by me as one of the oxidizing inhalants. It would have been absurd to enter into all these minute details in a popular work; hence the terms used by me in speaking of oxygen, were as follows:—“*What is that remedy? It is oxygen in such admixture with nitrogen, or atmospheric air, as shall best adapt it to the indications of the case,*” (p. 106). I never spoke of using uncombined oxygen; but oxygen combined either with nitrogen or atmospheric air. Now, observe his evidence—first, he says, it does not give off *pure* oxygen, and then that nitrous oxide would not, in his opinion, have the same effects as oxygen when, ignorant as he professes to be of chemistry, he must have known that it depended wholly upon the great volume of oxygen it gives up to the blood for its action on the system.

Even Dr. Williams is more candid than this for he speaks of oxygen and nitrous oxide as identical. Hear what he says in his book:—

“The oxygenating agents might perhaps be more efficacious could they be more readily conveyed into the blood without irritating the alimentary canal. The administration of oxygen or nitrous oxide by inhalation suggests itself as worthy of trial for the same object.”—(“Principles of Med.,” p. 169.)

These are the high-minded conscientious physicians who went

into the Court to destroy my reputation by quibbles and evasions. They are welcome to all the honour they will get from their performance. This denying that oxygen is oxygen is what Dr. Cotton, no doubt, meant by saying after the trial that he guessed "*they had put a spoke in my wheel.*" It requires, he will find, something more than such feeble ties as this to stop the onward roll of truth.

It is different; therefore it would not be the same, you will not go further than that; but is it not beneficial?—Oh, it is beneficial—it is one of the oldest remedies we know of, burning nitrate of potash in the chambers of those who are asthmatic. I recollect when I was a pupil of medicine it was very common indeed.

That has been done a good many years?—A good many years.

And it is supposed to have the effect in relieving the patient?—Yes.

The Lord Chief Justice. You mean in asthma?—Yes; sometimes.

In his evidence-in-chief he told us he had burned nitrate of potash "*several times.*" Now he tells us that he has done it ever since he was a pupil, and yet in all these years he has never even discovered what gas was given off. Even now he only *thinks* it is nitrous oxide. Is it possible to conceive a more humiliating confession for a medical man to make? Here, in the heart of London, surrounded by chemists, and yet administering a remedial agent for twenty-five or thirty years, concerning the nature of which he is all this time "*in doubt.*" If he was not able to decide the point for himself, why, in the name of reason, did he not submit it to some person who did know something of chemistry. What confidence can the public have in, or respect for our profession while such things exist. It is as discreditable as it is unaccountable. The only possible explanation of it is to be found in the fact that in England the time devoted to the study of chemistry is only half that which is considered indispensably necessary in other countries.

Mr. Coleridge. Can you account, assuming the fact to be true, for patients having been treated without permanent benefit, and their being treated by Dr. Hunter with permanent benefit?—As far as I recollect, the majority of them had not been treated by other persons.

That is your recollection?—I speak from recollection. I recollected that most of them gave their opinion that they were labouring under

asthma, which they looked upon as hopeless, and gave up themselves as being lost.

That is your recollection of what was stated by most of the witnesses?—No, by some.

Now, eliminate those, and deal with the remainder. How do you account for the remainder being permanently benefited?—I believe, taking the cases altogether, as we saw them yesterday, there is no man of any amount of practice who could not show an equal number of cases treated in the ordinary way of practice; I mean that there was nothing striking, not to my mind, at least.

But that is all you can say about it, assuming the permanency of the cure, that you think any other man might have done just as well?—Several of the cases admitted there was no permanency in the cure.

Well, they caught cold again. Dr. Hunter does not propose to insure against cold?—I was not criticising those cases, but they do not differ from the class of cases which any one of us might produce who has a fair amount of practice.

Now, it so happens that in every instance *except one* the patients had previously been under the care of other medical men, and *three* of them had been informed that their lungs were affected. He could recollect the one, but forgot the particulars of all the others. This reminds me of an anecdote which is told of a shepherd's boy. When asked by his master if he had counted all the sheep, "Yes" he replied, "I counted all except one little black lamb, and he frisked about so I could not count him." The only difference between Dr. Cotton and the boy is that he could count the one, but could not fix the other dozen.

You are still connected with the Brompton Hospital?—I am still physician there.

The pharmacopœia which you make use of there is divided into sedatives, alteratives, and so forth, is not it?—Yes.

And expectorants and astringents, and so forth?—Yes. We have not the head of expectorants.

But a competent medical man knows what an expectorant is?—Yes.

And, of course, a competent medical man knows what can be volatilized?—Yes, I should think so.

If told to give an expectorant in the shape of inhalation, he would know what expectorants could be given?—I should say not.

I must ask you how you explain that. If a man knows what are expectorants, and what are capable of being volatilized and what are not, what other condition is necessary to his knowledge before he can give an expectorant in the shape of an inhalation?—The word "ex-

pectorant" is one of considerable extent, and contains a vast number of subjects, so that if a man says in a book, take expectorants, he does not state anything. The same remark applies to sedatives.

You know what expectorants are?—Yes.

And you know what class of sedatives are capable of being volatilized and what are not?—When I read in this book that Mr. A B, whoever it may be, used an expectorant, I do not know what it means.

My question was very simple. You say you know what sedatives are, and I ask again do not you know perfectly well in the class of sedatives what can be given with vapour, and what not?—Yes, certainly.

And if you are told that, you say you know which can be volatilized?—I do not know which particular sedative is to be given. The mere statement that I am to give a sedative by inhalation does not tell me what particular sedative.

Would not that be left to the man who has to administer the principle to the particular case?—Certainly.

The Lord Chief Justice. It means this, that whatever sedative you think applicable to the case, and which otherwise give through the stomach, you are to give in the shape of inhalation?—Yes, I should suppose that is it.

Re-examined by Mr. KARSLAKE.

You are to find out for yourself what, according to his notion——

Mr. Coleridge. No; not his notion.

Mr. Karlake. What would be the particular sedative application to various cases?—I do not consider that this book informs me.

Assuming that up to the time at which this book was written you have given through the stomach, is there anything to show you in the event of a sedative being the particular remedy for the case what the proportions are for inhaling?—No.

I think you created a wrong impression in saying that in the Brompton Hospital pharmacopœia you divide your inhalants into different classes.

Mr. Coleridge. I did not ask the question.—I did not understand the inhalation; I understood the remedies.

Mr. Karlake. This is one of the most recent editions of your pharmacopœia (handing it to the witness)?—Yes.

I think there are about ten different sorts of inhalations mentioned there; I see it in the index?—Yes; ten.

And is the proper formula given, the number of drachms, and the mode of administering?—

I think you said that was rather for the dispenser?—Yes; we used in the Hospital—I speak for myself, but I believe my colleagues do also—other things beside those.

How old is the intrate of potash being burnt in a bed-room for the purpose of relieving asthma?—I have no idea.

Ever since you recollect?—Yes.

And it is applicable to asthma?—Yes; and cases of that class.

Now, as regards your book which has been quoted, have you any reason to alter the opinion expressed in it?—No.

The Lord Chief Justice. If I understand you rightly you put it scrofula and tubercle are only different forms of the same disease?—Different developments of the same constitutional disease.

Then your view is that unless there is constitutional disease, as either hereditary or developed in the course of life in this character, mere obstruction of the respiration would not result in consumption?—I believe so fully.

You altogether disbelieve the theory of Dr. Hunter that impaired or imperfect respiration will produce it?—Yes; I do.

It is a taint in the system, or else a predisposition of the system?—Yes; that is my view.

He says that this is his view, and as he was under oath we must accept it as final, but if so, what is the meaning of the following which I find in his book:—

“*Hereditary transmission* appears to have a smaller share in producing consumption than is generally supposed. In the thousand cases already spoken of, 367 or rather more than *one-third* were members of consumptive families, whilst the remaining 633 the disease *could not be shown to proceed from hereditary causes*” (p. 60).

It was according to his own showing about twice as often acquired without taint or predisposition as with it, and yet he in his evidence says, that taint or predisposition is necessary in every case.

He denies in his evidence that bronchitis and catarrh are a sufficient cause, and yet we find in his book the following:—

“Some persons although strongly predisposed, will exhibit nothing to justify the least apprehension until an apparently accidental attack of bronchitis proves the starting point of pulmonary consumption” (p. 89).

The truth is, I doubt very much if he knows what he does believe, or why he believes what he professes to believe. He seems to have had but one purpose in view in giving his evidence, and that was to *disagree* with everything Dr. Hunter had written, and he did it without the slightest regard to his own written opinions. Well, let him go. I have already wasted too much space and time refuting what really merited no more than a mingled feeling of contempt.

Adjourned to to-morrow at Ten o'clock.

FOURTH DAY.

DR. WILLIAM ORLANDO MARKHAM, *sworn*.

Examined by Mr. FITZJAMES STEPHEN.

Are you a Fellow of the College of Physicians?—Yes.

And also of the Royal Pathological Society?—I am a member of the Pathological Society.

You are a physician to St. Mary's Hospital?—Yes.

And also consulting physician to the Great Eastern Railway Provident Society?—I have been.

And the author of one or two medical publications?—Yes.

I presume you have read this work of Dr. Hunter's?—I have done so.

And having read the work through, will you state generally whether, in the first place, it states any system or theory as to the nature of consumption?—It gives a theory, and it gives an account both of the nature and of the treatment. It gives a theory of the nature, and also it gives the treatment of consumption.

With respect to the theory of consumption?—I take it that the general fair conclusion from the whole book is, that consumption consists of the deposit of carbon in the lungs, produced by the difficulty of the entrance of air into the lungs, and that as the result of this difficulty of the passage of air into the lungs carbon accumulates, and is deposited in the form of tubercle in the lungs. That, I believe, is the general theory, reading it from beginning to end, as a fair conclusion.

It will be seen that by this, Dr. Markham means to infer that I regard tubercle as *pure* carbon. When all I have said is, that it contains about 54 per cent. of carbon—leaving 46 per cent. to be made up of other elements. It is impossible that he could *honestly* have believed anything of the kind. But having transformed a *carbonaceous animal product* into pure carbon, he will proceed to demonstrate that that is untenable as applied to *tubercle*. The theory he defines, however, is *his* not mine. It is a *fiction* manufactured for the occasion.

Now, with regard to the soundness of that theory, what is your opinion as a physician?—I think it is utterly untenable by anything that is known in the way of scientific information that we can obtain, or that we can have.

Taking it by steps, do you think that consumption is caused by a

deposit of carbon in the lungs?—It positively is not; there is no such thing as a deposit of carbon in the lungs in consumption.

The Lord Chief Justice. In tubercle do you find any carbon except that which is common to the whole tissue?—You may as well call the muscle tubercle as call that tubercle. It would apply to one as well as the other, because their composition is almost identical.

Mr. Stephen. You do not consider that this deposit of carbon is caused by the restriction of the respiration?

The Lord Chief Justice. There is no deposit of carbon. You have tubercle like muscle, which contains a certain portion of carbon as one of the constituent elements; but carbon alone you find none.—No.

No, dear *Orlando*, it is not *carbon*, but does not tubercle contain a large percentage of *hydro-carbon*? Oh yes, that is true, no doubt, but I was speaking of *dry inorganic carbon*, and all I said was, that it was not *that*. Had his Lordship or the Counsel known the distinction I should have had to say *yes* instead of *no*, but they didn't, you see! It is true it misled them, and gave a false impression, but I don't think it was *perjury*—not *legal perjury*!

Mr. Stephen. That we put in this way. Does the obstruction of the air-passages cause the tubercle?—There is no reason to think so; in fact, it is quite the contrary. There is as much direct evidence to show the contrary, as in the well-known case of asthma, mentioned by Dr. Williams.

The Lord Chief Justice. I suppose if defective respiration would cause tubercle, it would be immaterial what caused defective respiration?—Quite immaterial as far as I can understand the theory.

Mr. Stephen. You have heard Dr. Bennett, who was examined yesterday. Do you agree with Dr. Bennett in stating that there are cases in which you get tubercle, although the respiration is free; that there are other cases in which you do not get tubercle, although the respiration is obstructed; and that there are other cases where you find tubercle where respiration could not affect the question?—There is no doubt about it.

And also that there are cases in which extensive deposits of carbon do actually occur in the lungs without tubercle resulting?—There is the well-known instance of what is called the miner's lung.

Now we come to the treatment. You have read the book through?—I have done so.

As you have stated in general what the theory of the book appears to you to be, as a medical man, now state what you gather, as a medical man, that treatment to be?—I think the fair conclusion from the whole book is, that the main theory in it is that the oxygen, which is sup-

posed or is given by Dr. Hunter's method, unites with the supposed carbon which is deposited in the lungs, and removes it.

The Lord Chief Justice. What he says is this, that the oxygen, artificially introduced, acts immediately on the carbon, and removes it, and that also it supplies the oxygen, otherwise deficient, to the blood, and so improves the character of the blood?—There is no doubt that it does; but I was referring specially to the removing of the tubercle.

In order to get the theory, I take that to be clearly what he means. You improve the blood by giving it the oxygen, and besides the local improvement, which would result from the improvement to the system, there is also the specific immediate action of the oxygen and the carbon?—Exactly.

What you have to deal with is the direct action on the local disease?—Yes; exactly so. I only mentioned that because we might admit the benefit of oxygen; that is, fresh air going to blood which is impure. But I may mention that there is nothing new in that, but the novelty is, that the oxygen removes that carbonaceous deposit in the lung.

The *direct* treatment alluded to in my book was, the *first*, the direct application of medicine to the inflamed mucous membrane of the air tubes; *second*, the direct action of the remedies and oxygen on the blood; and *third*, the direct action of the oxygen introduced into the blood on the hydro-carbonaceous products which we call *tubercle*. It will be seen that this is very different from the construction put upon my words by his Lordship and the witness.

Mr. Stephen. Will you state your opinion with regard to that point?—The thing is, I think it may be said in general physiological terms, simply impossible; that is, that the oxygen can act on the tubercle by the lungs directly, and by uniting with it; that is, by oxygenating it, remove it. It would be just as reasonable to suppose that the oxygen in the air could act on your finger, and remove that—that is, by acting upon the skin of your finger, unless you were to put it into the fire, and introduce the element of heat into it.

The Lord Chief Justice. That is, supposing your finger is a carbonaceous deposit.—It is so as much as tubercle.

That is not the assumption. Supposing you are right, and that tubercle is not a carbonaceous deposit, the theory clearly falls to the ground.—What do you mean by carbonaceous deposit? It is as much carbonaceous deposit as the skin or muscle.

Yes; because they all contain carbon as an element.—Yes; a certain amount of it.

What I understand him to mean is this, that there is so much of carbon separate from the other constituent elements of the tissue.

—Certainly; I suppose he means that; but tubercular deposit is not such.

Here the word "*separate*" is introduced, which alters the whole sense. As I never used such a form of expression, as "carbon separate from the other constituent elements of the tissue," it could only have been introduced by this witness to enable him to deny on oath that carbon exists "*separate*."

I know that. He states that it is, and assumes that it is.—Of course if he assumes it, and even if it were carbon, you could not remove it by the oxygen. The proof of that lies in the miner's lung. The miner has carbon, or what is the same, powdered coal—for that is carbon in a more or less pure state—in the lung, and no amount of oxygen taken into his lungs would ever remove that deposit.

Supposing you were to bring oxygen into immediate contact with the carbon at an ordinary temperature, what would be the effect?—It would have no effect.

The only form in which they unite is in the shape of carbonic acid gas, is it not?—Yes; under the influence of great heat.

Oxygen does not *directly* combine with carbon in the living body. It first combines directly with the hydrogen of the hydro-carbon and evolves heat, and this is believed by most chemists to be the chief source of the heat of the body. It then combines with the equivalent of carbon from which the hydrogen was separated, and forms carbonic acid, and eliminates it. When Dr. Markham talked of oxygen combining directly with the carbon of an organic substance in the living body, he must have been thinking of that "steam-engine and the Newcastle coals!"

Then of course that *great heat* would be an objection in Dr. Markham's views.

The "*great heat*" he spoke of, was no doubt thought by his Lordship to be a *great objection*, and that hence, Dr. Hunter ought to be *snuffed out*, lest he should set somebody on fire. *Nature*, is nothing to these modern wiseacres when they are playing at *scientific*!

The combination is carbonic acid gas?—Yes; undoubtedly.

And in the ordinary form in which carbon exists it would not form the gas?—Not without the influence of the vital organism.

Mr. Stephen. Supposing, for instance, you got as pure carbon

as you could get, and put it into a bladder of oxygen, would the oxygen have any effect upon it?—Certainly not.

Now, with regard to the manner in which Dr. Hunter proposes to administer oxygen, which you have heard described, can you state first what you apprehend to be this method of producing oxygen, and then whether you think that method would administer oxygen?—Do you mean from what fell from Dr. Hunter himself?

Yes; in court.—Yes, I can; and from his book I may say that I get no information as to the administration of oxygen.

The Lord Chief Justice. Not even from the book as explained by him, or his evidence; or do you mean from the book alone?—From the book alone I get no evidence whatever. But I was going to observe from what I heard fall from Dr. Hunter as to the administration of particular things which I understood him to give, and giving them as oxygenated compounds—speaking from the ordinary chemical knowledge of a medical man, I believe that there is not one of them from which any oxygen whatever is given off, excepting only in the case he mentioned of where he gives it in a chamber, with the manganese and the sulphuric acid, which is the ordinary old-fashioned mode of producing it; but none is given off in those inhalations which, as he told us, were ordinarily given to the patients.

The Lord Chief Justice. The book in fact affords no information as to how oxygen is to be administered.—No.

Mr. Stephen. Is there any passage in the whole book—I will put it generally—which would give you any information as to what vapours are to be inhaled, how they are to be compounded, or when compounded how they are to be administered?—I believe he gives in some part a general list, putting them down under the heads of Sedatives, Expectorants, and Alteratives. I suppose that a doctor might take a general idea from that of what was meant by those words.

The Lord Chief Justice. What should you understand by an alterative inhalant?—As far as an alterative inhalant goes I should be puzzled to know what it did mean, except that it comprised all the others, because you might put them all down under the head of alteratives, according to the manner in which you used them.

You would not know what specifically was meant by the term?—Certainly not.

Mr. Stephen. With regard to the reports of cases in the book, would they give you any information as to the matter—the medical directions?—None whatever; I am not sure whether there is one that speaks of taking oxygen.

Mr. Karlake. Not alone?

The Witness. As a medical record of a case which would give a medical man information, they are utterly worthless.

Mr. Stephen. Now with regard to the practice of inhalation itself, what do you say as to that; is it a thing which has been used by the medical profession?—I remember when I was a student thirty years

ago in a country hospital, that I superintended the giving of it, and from that day to this I have never known it not used in ordinary use either in one way or the other.

The Lord Chief Justice. But not with oxygen?—Not with oxygen.

Mr. Stephen. Can you form any opinion as to the administration of oxygen?—I have endeavoured to get some information from the books on the subject; but as far as the literary history of it goes I find that there is nothing at present to show that it is of the slightest benefit, but rather that the contrary* is the case; and I cannot imagine beforehand anything that would induce me to use it without having some positive clinical proof—that is, some series of cases, the result of which would give me reason to think it would be useful.

You mean the changes that have occurred under treatment?—Yes; such a clinical record of the history of the case as would be of value.

The Lord Chief Justice. Judging *à priori* from your knowledge as a medical man, would you form a favourable opinion?—I would not like to give any opinion upon the subject. I cannot anticipate any good from it. We have already more drugs than we know what to do with, and I should not be inclined to use it without I had good reason for doing so.

Mr. Stephen. Do you think that inhalations from conium, stramonium, and the various other things that have been mentioned, would be beneficial in cases of bronchitis and asthma?—I do not doubt it; in fact I am sure of it.

Is that a fact well known to the medical profession?—I think it is as well known as any fact is known.

Bearing that in mind, and referring to the patients who gave evidence yesterday, do you think that there was anything novel in their cures?—I think they were very good specimens of cures; but I think there are very few doctors in London who could not give as good an account of the history of their cases as those patients did.

I suppose most doctors cure sometimes?—It is to be hoped so, as well as the others.

You heard the patients describe their symptoms. Were any of those symptoms, so far as you could judge, symptoms of pulmonary consumption?—No, certainly not.

The Lord Chief Justice. I do not understand that it is suggested that they were.

Mr. Coleridge. Nobody said they were.

The Lord Chief Justice. We have had no case of pulmonary consumption.

The Witness. The nature of their disease was shown in most of their own persons as they stood in court.

It is quite clear that no cases, that I may succeed in restoring to health, will ever be admitted by these medical witnesses to be cases of consumption. Some of the patients

had cough, expectoration, shortness of breath, and loss of flesh, united with spitting of blood, hectic fever, and night sweats, and had been pronounced to be in consumption by other medical men. But what of that? Dr. Markham does not hesitate to say that not one of them were consumptive, though he had never *examined* them, nor had he even seen them except in the witness-box. This was quite in keeping with the spirit manifested throughout. They would not admit that any of these were cases of consumption, because, to do so, would not harmonize with the objects these scientific experts had in view! Still it must be admitted, even by them, that it was a good thing for the patients to get rid of their disease, and that it could not be a bad practice which enabled them to do so, after all other means had been tried in vain.

Mr. Stephen. I do not want to take you through all these passages, but I will ask you generally. You heard the passages read about catarrh and elongated uvula, about the pulse, and about the young women; do you agree with what was said upon them by Dr. Bennett and the other gentlemen who have been called upon those matters?—Certainly; entirely so. I can only look upon them as simply little methods of frightening the public.

How *little* men do differ from *great* ones! Hear now what Sir James Clark says on the necessity of awakening the public—

“I cannot but be aware of the great difficulties which present themselves to the accomplishment of my views regarding the prevention; and that these can never become generally applicable *until the public is fully impressed with the necessity of attending to them.* This furnishes me with another urgent motive for the publication of the present work.”—*Clark on Consumption*, preface, p. xi.

Again—

“There being also a probability that my work, from the importance and general interest of the subject, may pass into the hands of the public, I have endeavoured to divest my language as much as possible of technical terms.”—p. xii.

What do you say about the bleeding?—It is a pure exaggeration.

Mr. Serjeant Parry. I do not think he should say that. It shows the temper and disposition.

Cross-examined by Mr. COLERIDGE.

You generally agree with these eminent men, and generally disagree with Dr. Hunter, I suppose?—With regard to those passages.

And I think I caught you saying that they were merely modes of frightening the public?—Just so.

You have considered the matter of consumption and tubercle a good deal?—Every physician has done so, and indeed could not help doing so.

I asked you whether you had done so?—Certainly.

What is it?—A consumption, as far as we know——

Tubercle is what I asked you?—Consumption, I say, as far as we know anything——

Tubercle was my question.—You asked me about consumption.

I asked you first about tubercle?—Consumption, I was going to say, is simply tubercle in the lungs.

But what is tubercle?—Tubercle is an albuminous compound, consisting of carbon, nitrogen, oxygen, and hydrogen.

Here he differs from Dr. Williams, who says it is a “*fibrinous*” compound.

Are people agreed about tubercle?—In what respect do you mean?

As to what it is, or as to what makes it?—Yes, certainly; ultimate analysis tells us that without doubt.

Then you would not agree with this: “That the opinions of the best,” &c. [reading down to the words] “of protein?”—I understand it. All those compounds are nearly the same in element; that is, as far as their elementary composition is concerned they are the same.

The Lord Chief Justice. In somewhat different proportions, they all contain the same elements?—Every one of them contains the same elements.

But it happens to be these “*different proportions*” which make all the difference between *health* and *disease*. A very slight difference alters the whole nature of a substance. We see this specially exemplified in chemical compounds. Two simple agents will unite to form one *active* one, or the same agents combined differently make a *harmless* substance in one case, and a deadly poison in the other.

Mr. Coleridge. Almost all animal substances contain the same things in different proportions?—Not all.

The greater proportion of them?—The greater proportion of the soft tissues.

They contain or are made up of the same elements in different proportions?—Very nearly the same elements.

Differently proportioned?—Differently proportioned.

In any organic substance the difference will, generally speaking, be as to the relative proportions of the ingredients, will it not, if there be a difference?—It is merely a matter of chemical analysis. The diffe-

rence between tubercle and flesh, or albumen, is almost nothing. The difference may be said to be so slight as in ultimate chemical analysis to render the things the same.

There is carbon and oxygen, and hydrogen, and nitrogen, in all of them?—Yes.

Only that they are in different proportions?—There is hardly any difference in the proportions. If there be, it may be 53 or 54 of carbon; that is, only one or one and a half difference between the two, and that slight difference, I believe your client will tell you, is in the ultimate analysis so small as to render them almost the same.

Do I understand you to say that carbon does not in truth accumulate, and that it is not prejudicial to life when it does accumulate—that is, when the blood, for instance, or the tissues have too much carbon in them?—I do not know any case where it does accumulate as such in the tissues.

“*As such*,” are here thrown in to avoid saying that it does accumulate. It does not accumulate “*separate*,” and it does not accumulate as “*pure*” carbon. All these are *saving* words, added by this conscientious witness to avoid admitting a fact. He might as well have *assumed* that I meant *diamonds*, and then testified that tubercles were *not* diamonds.

As such?—As carbon.

Is carbonic acid the same thing?—It is a totally different thing; there is as much difference between carbon and carbonic acid as there is between mercury and opium.

Then the blood does not get carbonaceous, and that is not a bad thing?—Perhaps you will explain what you mean by carbonaceous?

I cannot do that. Pray do not examine me in chemistry.—I do not understand the terms of the question.

Here he seeks to evade the question by asking the counsel to define what he meant by “*carbonaceous*,” knowing that the counsel could know very little about the subject, it being purely *medical*. We shall see that he *did* know very well what was meant, by his answer to the next question.

Is there no such thing as carbonaceous blood?—If you mean that a large amount of carbonic acid or carbon may accumulate in the blood in the shape of carbonic acid, I admit it. There may be more carbonic acid than ought to be there.

Is it not true that the restorative process which the lung gives to the blood is that of replacing the proper quantity of oxygen that should be in the blood?—Yes, undoubtedly; it is simply breathing.

The result of breathing is to oxygenate the blood?—It is rather

a deep physiological question, and it is not quite clear where the oxygen unites with the carbon.

The Lord Chief Justice. The fact is that the breathing passes a certain quantity of oxygen taken from the air into the blood?—Undoubtedly.

What combination takes place, or where that combination takes place is not exactly known?—Where it takes place, or where carbonic acid is given off.

Mr. Coleridge. I thought that unless a proper amount of oxygen was imparted into the blood, that too much carbon would remain in the blood?—Undoubtedly we will say so; it would be in the blood.

I do not want you to say so unless it is so; but is it so?—Undoubtedly the carbon would accumulate.

And being there in a large proportion it would injure the blood?—I do not think that carbon is ever free in the blood as carbon; it remains as carbonic acid, as a gas.

I supposed the carbonic acid of the venous blood was chiefly, if not entirely, held in alkaline *solution*, and did not exist as “a gas” until it was eliminated in the capillaries of the lungs; but we are continually learning. Of course, Liebig, Carpenter, and all other physiologists will confess *their* error, and introduce Markham’s *new* physiological fact!

The Lord Chief Justice. He does not acknowledge that it exists *qua* carbon; it only exists with oxygen in the shape of carbonic acid gas.

Mr. Coleridge. Yes; but carbonic acid has not got enough oxygen in it?—It has got its amount, because it would not be carbonic acid if it had more.

If the blood is not sufficiently oxygenated it has carbonic acid gas or whatever you please to call it, in too large a proportion?—Yes.

That is the substance of it. Then the good that is to be done to it is to put oxygen into it?—And remove it.

That is the substance of it, call it what you please.—That is the substance of it.

The Lord Chief Justice. Carbonic acid gas is formed, and you exhale it?—Yes.

Mr. Coleridge. And that turns the venous blood into artificial blood?—It does.

Would not the obstruction of the air passages of the lungs prevent the requisite quantity of oxygen getting into the blood?—Certainly.

And thereby do damage?—Yes.

Would not anything that tended to open the air passages and increase them, and convey more oxygen into the blood, be useful?—Undoubtedly.

You say you think this system of Dr. Hunter’s does not do anything of the kind?—No, excuse me; I never said that.

The Lord Chief Justice. He never said anything of the sort.

Mr. Coleridge. I thought I understood you to say that what Dr. Hunter professed to do did not convey oxygen into the blood, and did not thereby do any good?—I said that the method which he said he used to administer oxygen did not produce oxygen.

The Lord Chief Justice. And also that the effect of the oxygen on the local mischief would not have the effect which he describes?—Certainly.

Mr. Coleridge. If I have misrepresented you at all, I am sure I regret it; it was not at all intentional. I understood you to say that Dr. Hunter's method did not do what it professed to do; that is, that it did not convey oxygen into the blood, and thereby do it good when it wanted it?—Undoubtedly I said so.

I thought you said that his method professed to do what it did not do; namely, convey oxygen into the blood, and redress the balance of oxygen and the carbon, or whatever it may be made up of. That is what I understood you to say in substance?—In substance, simply because his method did not give oxygen at all.

Just so. You admit that if it did give oxygen it would be so far good?—I am not quite clear upon that, for the simple reason that I do not know that the lung could take up more oxygen under those circumstances than it could do out of the air. From the air that you respire, as everybody knows, there is only one part out of the six, during respiration, that the lungs take up; so that there is always an immense deal more oxygen given to the lungs during respiration than they actually require.

Than they take?—Yes; I am not sure that the lungs could take up more even if it were given.

But if they could, it would be according to theory a good thing?—I should really not like to give an opinion upon the subject, for I am not quite clear upon it.

It will be remembered that both Dr. Cotton and Dr. Risden Bennett gave it as *their* opinion, that oxygen would "*excite*," and would be likely to increase the animal heat, and might do a great many things. Now we have another one of these scientific gentlemen, and he tells us he doubts whether oxygen would be taken up at all! If it would not be taken, how could it "*excite*" the patient? and if it would excite, must it not be taken up? Really, these differences of opinion are very sad. What *can* the public think, except that Dr. Markham, or the other medical men, or probably all of them, were giving their opinions on a subject which they did not understand?

Then he does not agree with Dr. Williams, who cer-

tainly led the Court to suppose that he used oxygen, and that others had used it. The only thing that I regard as conclusive on this point is, that Dr. Williams had repeatedly *thought it might be used*, and that Dr. Markham was not sufficiently informed on the subject. Had he been otherwise, he certainly should have known that the late Dr. Riadore recommended and used it, and that Dr. Birch also had written a book strongly recommending it in consumption.

Do you say really that in all these substances, that is different things that Dr. Hunter gives by inhalation, there is none, as far as you know, that gives off oxygen?—Certainly not. I mentioned one exception, that was the peroxide of manganese; but that must be made in a special chamber, and superintended by an artist, but I am speaking of the ordinary methods which he gives.

Did you hear Mr. Curtis, the manager of Messrs. Corbyn, examined?—Yes.

Did you hear him say what he supplied to Dr. Hunter were chiefly things that evolved oxygen?—He said that they were oxygenating agents, I think.

I will not pledge myself to the exact words, but my impression is that he said he sold chloric acid and other substances which evolved oxygen. According to my recollection he said so; but if I am wrong that must be a mistake.—Yes, an entire mistake, except with regard to the chloric acid.

Yes, but that was a separate point. It was a mistake if he meant anything more than chloric acid?—If he spoke of nitrate of potash, and if he supposed that the burning of that produced oxygen, that would be an entire mistake. The same with regard to chloride of potash, it would be a mistake.

It produces nitrous oxide?—Yes; or something of that kind.

Is that an equivalent of oxygen?—Certainly not; it is no more oxygen than mercury is opium.

Is it an equivalent of oxygen?—It is a totally different agent.

He says it is “something entirely different from oxygen!” There is no more difference than there would be to put a teaspoonful of brandy into a tumbler full of water in one case, and a tablespoonful in the other. The first mixture would be weak and the second strong. The difference would in each case depend upon the quantity of the brandy, the water being estimated merely as the diluent. Was this evidence such as a candid straightforward man would have given? Was it not a “scientific” quibble intended to injure me, to mislead the

Court, and to conceal the truth. He is welcome to all the honour it will bring him ; but I cannot help reminding him of a work which I fear did not enter into the catalogue of his school books. He will find the 15th, 16th, and 17th chapters of the 1st part, and the 1st, 6th, 7th, 8th, and 12th chapters of the 2nd part of Book iii. *Paley's Moral Philosophy* very suggestive reading.

But to return to matters of fact. Dr. Williams evidently does not think nitrous oxide an entirely different thing from oxygen, but quite identical with oxygen in its action. He says, "the *oxygenating* agents might perhaps be more efficacious could they be more readily conveyed into the blood without irritating the alimentary canal. [Stomach, &c.] The administration of oxygen or nitrous oxide by inhalation, suggests itself as worthy of trial for the same object." (*Principles* p. 169.) Here it is seen that "oxygen or nitrous oxide" are spoken of as *identical* in their action. Dr. Markham says that nitrous oxide is not only a different agent from oxygen, but is as different as "mercury from opium." Now mercury is a mineral, and opium a vegetable product. The one soothes the nervous system, and the other purges the system and irritates the nerves. They have, in fact, nothing in common nature, properties, or physiological effects. The one may be said to be the very opposite of the other. Now compare oxygen and nitrous oxide : they are both gaseous—both produce precisely the same action on the blood and on the health, and both depend on the same element for their action ; and that element is oxygen, the one being pure oxygen, and the other oxygen mixed with nitrogen. There is no possible escape from the inference that Dr. Markham has on *elastic conscience*.

Does it do the same thing or perform the same function as oxygen ?
—Which agent ?

Nitrous oxide.—Do you mean the protoxide of nitrogen—the laughing gas ?

I mean what you get from saturating paper with nitrate of potash, and burning the paper ?—Nitrate of potash is a different matter. Nitrous oxide is gas ; what you are speaking of is common salt-petre.

If you saturate paper with nitrate of potash, and burn it, does that evolve oxygen?—Certainly not.

Does it evolve nitrous oxide?—It evolves some of the gases.

Is that an equivalent of oxygen?—Certainly not.

Does it perform the same function as oxygen would?—I never heard of it doing so. It was used by Mr. Beddows, and as laughing gas to amuse people.

Is it not used in cases of asthma?—The burning of saltpetre is a common method of filling a room with vapour, for the purpose of benefiting the patient.

The Lord Chief Justice. What is the vapour that does good?—I am not clear about it; whether there is a small quantity of nitrous acid in it I do not know—but there is no free oxygen at all events in it.

To act on the lungs it must be free oxygen?—Of course; it cannot be oxygen without being free.

You are quite right; but you must have it in the shape of oxygen, separate from any combination into which oxygen enters before it can be taken up by the lungs and passed into the system?—Undoubtedly, passing in as such as oxygen.

If he meant what he is understood to mean, the statement would be absurd, and the very opposite of the truth. But it is a mere quibble. Read carefully his last reply, and you will see “*as such as oxygen.*” Although not very good sense, still he means that nitrous oxide is not pure oxygen, and it does not pass into the lungs “*as such*”—i.e., as *pure* oxygen. But he does *not* mean to say nitrous oxide will not give up an increased quantity of oxygen to the blood; he does not mean to say that the oxygen derived from nitrous oxide is not the same in its properties as oxygen derived from the air. If he does, then his evidence on this point is against all experience and authority, and directly the opposite of Dr. Williams’ opinion.

Mr. Coleridge. I think you say with regard to the cases you heard of the day before yesterday, that you think any doctor could give an equally good account of his cases?—I have no doubt of it; he could put just as many into the case.

Supposing those cures were permanent, or at all events, had lasted for a considerable time, how do you account for their not having been successfully treated so before? Do you account for it in the same manner as one of the witnesses did yesterday—that they had not been treated at all before?—No. I do not say so; but if you will allow me to go to my hospital I will bring twenty patients who will tell you the same tale—that they have been under other doctors, and found no relief. But if you took a hundred consecutive patients of mine you

would hear a very different tale, and I daresay you would of Dr. Hunter's.

That is the common experience of doctors?—It is undoubtedly common experience. I am speaking of a large mass of patients, such as you see at hospitals.

Would you have treated them much as you understood Dr. Hunter treated them?—Yes, I should have done so.

Re-examined by Mr. KARSLAKE.

You want the real oxygen of the air to breathe, and if you have something which has oxygen in it, that is not an equivalent for oxygen?—Certainly not; it is a totally different thing.

Then the carbonic acid is what you want to get rid of in the lungs, but it is generated by mixing oxygen with the carbon?—Carbon is perfectly innocent, if it were in the lungs. I can imagine it being there in a perfectly innocent state, as in a miner's lung; but carbonic acid is a deadly poison if it accumulates.

(The witness was directed to withdraw.)

DR. GEORGE JOHNSON, *sworn*.

Examined by Mr. FITZJAMES STEPHEN.

I believe you are a physician of medicine of the University of London?—Yes.

Mr. Coleridge. Perhaps your lordship will allow me to ask the last witness, Dr. Markham, a question which I omitted to put to him?

The Lord Chief Justice. Yes.

Mr. Coleridge (to Dr. Markham). Were you consulted before the libel was written?

Dr. Markham. Assuredly not.

Mr Coleridge. I have a reason for asking it, my lord.

The Lord Chief Justice. If I had known what the question was, I should not have allowed it to be put, especially coming as it did irregularly.

Mr. Coleridge. I had no right to assume that he was not consulted, if I did not ask him the question. I do not think your lordship quite understands the object of my question. Believe me, my lord, that I did not put it with the view which probably is supposed. I assumed that the answer would be what it was. I did not put the question with the view of imputing anything to the witness, but for a totally different reason. I should not have a right to assert that all these gentlemen were not consulted upon the libel, unless I were to put the question distinctly to them in the manner in which I have done.

Mr. Karlake. If you ask me to admit that, I should have no hesitation in doing so.

Mr. Coleridge. Well, if I may take it as an admitted fact that none of these gentlemen were consulted by the writer of the article before the libel was written, or had anything to do with the subject beforehand, that will suit my purpose quite as well, and render it unnecessary for me to repeat the question.

Mr. Stephen. Now, Dr. Johnson, you are a doctor of medicine and a Fellow of the College of Physicians, and a member of the Senate of the University of London, and also a Fellow of the College of Surgeons?—Not of the College of Surgeons.

You are the author of a variety of works? Have you read this book of Dr. Hunter's?—I have.

Now I will ask you generally, with regard to the theory of consumption which it lays down, what is your view upon that subject?—I think that the theory cannot be maintained. Undoubted facts are entirely opposed to it. I may state very briefly that the facts have been already so plainly stated by Dr. Williams and others—I mean with respect to tubercle depending upon defective oxygenation of the blood—that perhaps I scarcely need repeat what they have said. But the very numerous class of cases in which, for successive months and years, patients suffer from defective oxygenation of the blood, heart disease, bronchitis, and all that class of cases, and yet very rarely become tuberculous, is, I think, alone sufficient to disprove that theory.

I apprehend from that answer, after reading the book as a medical man, that you agree with Dr. Markham as to what the theory of the book is?—Yes; that tubercles depend upon defective oxygenation, and a consequent accumulation of carbon.

Do you agree also as to the means which he proposes to take for removing tubercle from the lungs?—Yes; that is, as to the process of inhalation, and especially the administration of oxygen, which is the theory of the treatment.

Supposing the theory to be correct, that in point of fact tubercle was a carbonaceous deposit in the lung, should you say that the introduction of oxygen into the lung would remove it?—No; I think it quite impossible that it should.

He thinks if tubercle were admitted to be a "*carbonaceous product*," oxygen introduced into the blood would not remove it. And yet oxygen is of all agents the most subtle and powerful in *nature*, and has, moreover, a direct chemical affinity for *carbonaceous* compounds. If oxygen will not remove it, no other agent in nature is likely to do so? Hear what Dr. MacCormac says on this subject—

"*Oxidize* the effete carbonaceous waste, and it is got rid of and disappears. Do "not oxidize it, and it will remain in the organism under the guise of tubercle, "and no other.—p: 116.

This is precisely my idea, and the aim of my practice is to oxidize the carbonaceous compound. Dr. Johnson does not agree with me, and differs quite as much from Dr. MacCormac. In fact, it is a case of "*doctors disagree.*"

Then, setting aside that matter, what do you say with regard to the means taken by Dr. Hunter, as described in the box yesterday, for the administration of oxygen? Do you think that they would produce oxygen?—No; I think not. The chief means mentioned by Dr. Hunter himself, namely, the adding of chloric acid to boiling water, combined with other vegetable substances, stramonium for instance, and other things, I think that the effect of that would be that scarcely any, if any, free oxygen would be given off.

But supposing chloric acid alone were given?—Chloric acid alone would decompose, give off chlorine in combination with oxygen.

And chlorine is a very irritating substance, I believe?—Very. It is a gas which it is scarcely possible to breathe in a concentrated form. It must be very diluted indeed before you can breathe it.

On the other hand, if you added the various other things which have been mentioned, would there be any oxygen evolved?—No; then the oxygen would at once combine with those organic vegetable matters.

So that it could not be taken alone, and could not be given in composition?—Yes.

Now, apart from this special theory, what would you say as to the administration of oxygen in cases of consumption?—I should not expect any benefit from it. I have had no experience in the administration of oxygen by means of inhalation in consumption; but theoretically there is no *à priori* reason for expecting good from it, and I am not aware that any experience has proved that good has resulted from it.

Are you of opinion that any *à priori* reason would show that in a certain class of cases it would be dangerous?—Yes, if largely given it might act as a stimulant, and excite the circulation; and as suggested by other physicians, it might cause bleeding by a rupture of the blood-vessels.

The Lord Chief Justice. With regard to that observation of yours, rupturing the blood-vessels, can such a thing take place?—Certainly, my lord.

The reason why I ask you is, that Dr. Hunter, in his book, denies the fact that rupture of a blood-vessel can take place.—It is a very strange assertion, my lord.

Well might the witness say, "it is a very strange assertion, my Lord," but it was "*my Lord's*" assertion, not mine. Such an absurdity never was said or written by me. I said, the

popular idea is that a blood-vessel breaks every time a patient spits blood; that this is not true—that the blood is “*exhaled*.” This is not only my view, but it is also the opinion of the great Laennec, to whom the profession is indebted for the discovery of *auscultation*, and for almost all that is known of the nature of tubercle. Then it is also the opinion expressed by Sir Thomas Watson in his Lectures, published only eight years ago. I prefer Laennec, and Watson to Johnson, and what is more, I prefer to cheer and encourage my patients by telling them that they are in no danger of bleeding to death—that blood-vessels do not break under such circumstances—not that they *never* break! Dr. Johnson thinks it better to tell them they have broken a blood-vessel every time they find a streak of blood in the sputa. Which of these opinions is the most “*alarming*?” to which shall we apply the word “*terrorism*?” If I had in my book expressed precisely the opinion given by Dr. Johnson, then they would have produced the works of Laennec and Sir Thomas Watson to prove that blood-vessels do not break. So that whichever way the matter might have been, *I* should have been *wrong*!

I think I saw it somewhere in his book.—I think he rather suggests that in the ordinary case of bleeding no blood-vessel is ruptured; that if any blood-vessel of any size were to burst, there would be no stopping it, and the hæmorrhage would be fatal.

Mr. Stephen. Perhaps your lordship is referring to page 52?

The Lord Chief Justice. Yes; that is it. “It is a common belief that the blood comes from the breaking of a blood-vessel, and this idea is entertained by many physicians. This is no more true than it would be to say that bleeding from the nose comes from the breaking of a blood-vessel. The blood merely oozes through the coats of the relaxed vessels, in medical phrase we say it is exhaled. Should a vessel of any size actually break, and such cases do occur, though extremely rare, the patient necessarily bleeds to death, because neither medicine nor surgery affords us any means by which we can apply either a styptic, torsion, or the ligature. But as this misfortune has never occurred to me in many thousands of cases of consumptive disease, its danger is not worth considering.” When I was reading that I was rather surprised to hear that a case of bleeding, which one has so often heard of, was likely to end in a state of consumption, and result in the patient dying.

The Witness. It is physically impossible that hæmorrhage can take place without breaking a blood-vessel. The blood consists of cor-

puscules which are of visible size, and the blood-vessels contain no pores; that is, there are no holes through which the corpuscles could pass. If you take bleeding from the nose, for instance, there must be a rupture of a blood-vessel somewhere, of small size, no doubt, but still it is the actual rupture of a blood-vessel.

The Lord Chief Justice. What the plaintiff says is, "Should a vessel of any size actually break, and such cases do occur, though extremely rare, the patient necessarily bleeds to death."—That is not true. A blood-vessel of considerable size may break, and yet have no fatal termination; but the vessel will heal of itself, even although no surgical means can be used to stop the bleeding. Even where there is hæmorrhage in a vessel of largish size, the blood accumulates about the mouth of the ruptured vessel, and it will cease.

Mr. Stephen. Would relaxation of the blood-vessel, supposing it to be relaxed, open pores in the blood-vessel itself through which the blood corpuscles could pass?—Certainly not. It is physically impossible that bleeding can take place without a rupture of the blood-vessel.

There is another passage in this book with respect to which I wish to ask you a question. I think you have made a special study of chronic laryngitis?—I have studied it.

Will you look at page 21?—Yes.

There is a passage there about chronic laryngitis. It begins thus:—"From whatever cause it arises, chronic laryngitis is always a serious malady, for it threatens the complete destruction of the voice, and when left to itself rarely ends until it has involved the lungs in disease. It will not die out of its own accord, for every slight cold, every change in the weather, every flight of dust feeds the irritation and increases the inveteracy of its hold. In all cases it must be treated by the direct application of astringents and alteratives to the affected part. Like catarrh and granular sore throat, it is a local affection, and can only be cured by local treatment. There is no use in torturing the poor, unoffending stomach for an inflamed condition of the vocal chords of the larynx. Change of air is equally unavailing, for there is no climate without dust and drafts of air sufficient to feed and keep the disease alive. The patient must use the inhaling instrument morning and evening, charged with such sedative, alterative, and astringent medicines as may be indicated by the stage of the disease, and fumigate the larynx every night before going to bed with warm sedative vapours, or he will go on from bad to worse, until cure is impossible." Have you any observation to make upon that?—I would remark that the writer of that paragraph appears to have had more regard for the unoffending stomach than for the unoffending lungs. He speaks of chronic laryngitis as being a local affection, and to be treated by local remedies; but his means of treatment is by the inhalation of various sedative, alterative, and astringent medicines. It is quite certain, of course, that these inhalant materials are not limited in their action to

the larynx; they pass on into the lungs and into the blood, and entering the blood they may affect various distant organs. Every one who has used inhalation knows that after very unpleasant results follow from the inhalation of drugs. The brain is often disturbed by the drugs entering the circulation; so that it is a great mistake to suppose that the inhalation of drugs for the cure of chronic laryngitis is purely local. The remedies go much beyond the point to which they are intended to go. Then it also occurs to me to remark, if I may be permitted to do so, that here is a book on the throat, the larynx, and the lungs, and there is no indication throughout the book that the author has used that most valuable instrument, the laryngoscope—a mirror by which he can see the interior of the larynx just as plainly as he can the tongue or the tonsils. It is of great value, not only as showing the condition of the larynx, but by enabling the practitioner to apply local remedies with great ease and accuracy. Not only can you see any ulcer that may be in the larynx, but you can apply local remedies to it. Then the treatment is a purely local treatment; so that, unless the practitioner uses this instrument, and applies his remedy in that way, his treatment of the larynx is not merely local, but he may do great harm to the lungs, and he tortures the unoffending lungs in his endeavour to cure the larynx. It appears to me remarkable that a book of this kind should make no mention of such an instrument, not only as a means of diagnosis, but also of treatment.

The Lord Chief Justice. The instrument enables you to use a local remedy, and pass things into the larynx itself?—Yes, with great ease. You can guide the remedy by the mirror. It is a small mirror that has been warmed to prevent the breath from dimming its surface, and it is held at the back of the throat, so that you get a perfect view of the interior of the larynx, and it enables you to apply local remedies to any part of the larynx with great precision.

It is amazing with what perfect unanimity they all agreed that tubercle was not “*charcoal*,” as though any person versed in animal chemistry had ever supposed or asserted it to be so. Then, knowing that I had said I did not use “*pure oxygen*,” how he dwells on the fact that I could not get “*pure oxygen*” from the compounds used, as though I wished to do so. In my evidence I distinctly said pure oxygen was less efficacious than “*combined oxygen*.” Again we have the clap-trap about chlorine being “*irritating*,” and that pure oxygen *might* be injurious. It was not that it *had been*, or that he had seen any injury result from it. All this, therefore, was merely intended to frighten people from becoming my patients—a piece of *organized medical terrorism*, probably agreed upon by

these high-minded gentlemen before going into the witness-box, as a means of depriving the afflicted of the benefits of oxygen inhalations. Its direct effect, and its obvious purpose, was "terrorism."

The only other point to which I think it worth adverting in this witness's evidence, is one personal to himself. He evidently wished it to be known to the world that he uses the Laryngoscope, and has for some time past paid great attention to the treatment of chronic laryngitis. He voluntarily introduced the matter apparently for the sake of boasting of his knowledge and skill. I had not alluded to the laryngoscope in my book. Had I done so, and with some complimentary remarks, he might not probably have appeared in Court against me armed with the laryngoscope to damage my reputation, and blow his own trumpet. Now it so happens that I do know all about the laryngoscope and but very little to the credit of the practice which has sprung from its use. I have no confidence in it as a means of diagnosis, and I am decidedly opposed to torturing the larynx with strong applications of any kind. I am convinced that they often do great harm, and very seldom accomplish any good.

Since I came to England the many courtesies I have received from my professional brethren of his class, led me to inquire into the results of their practice. I found that the deaths from laryngitis had enormously increased—in fact had risen from 30 in each million of the population to 347—and that where 470 only died twenty years ago, now upwards of 7000 die! That, whereas the increase of the population is only 29½ per cent., the deaths from this disease have increased *fourteen hundred per cent.* Now these facts were not such as to inspire me with much confidence in the laryngoscope or its advocates. Since I know that Dr. George Johnson is one of them, I can assure him, I much prefer to have him for an adversary than that he should have me for a *patient*!

Mr. Stephen. I do not want to take you through all the passages in detail; but your attention has been called to the passages about caustic?

The Lord Chief Justice. He speaks very strongly against the application of such remedies to the larynx.

The Witness. He does so.

Mr. Stephen. What do you say about that?—That they are of undoubted value.

Do you think that applying lunar caustic to the uvula would ever cause consumption?—I have frequently seen the caustic heal a scrofulous ulcer in the throat.

It is not within your experience that caustic ever sets up something which terminates in tubercular consumption?—Certainly not.

What do you say as to the statement about elongated uvula causing an irritation which produces consumption?—I never knew such a result, and I think it very improbable that it should occur.

Do you think it reasonably possible?—Not at all.

I suppose it is not an uncommon thing for people to feel a tickling in the throat from elongated uvula?—It is very common, and very annoying.

It would be a very alarming state of things if you were told that such a thing as that would cause consumption?—Yes.

You have heard the passages read that relate to that?—Yes.

The Lord Chief Justice. The elongated uvula is a very common throat affection, is it not?—Very.

Mr. Stephen. You have seen the passages about catarrh leading to consumption?—Yes, I have seen the passages.

What should you say about that?—I should say that it never leads to consumption. Cases may begin with symptoms of catarrh, but out of the vast number of cases of catarrh that occur and exist for a long time, it is an extremely rare occurrence for any one to pass into consumption.

So with respect to neglected colds, and so on?—Certainly, they do not pass into consumption as a rule.

Now, I will ask you generally about what has been said about the pulse, and the passage about the young women, and those other passages which have been read several times. Do you agree in the opinion which the other medical witnesses for the defendant have expressed on those passages?—Yes.

What do you say with regard to the treatment by cod-liver oil, and tonics, and so forth?—I believe that such treatment is of great use.

Do you think it has the effect of lengthening the life of consumptive persons?—I should think it highly probable.

Have you had much experience with respect to inhalation?—Yes; I have been in the habit of employing inhalation for many years.

Would that be the treatment in respect of asthma and bronchitis that should be adopted?—Yes, it would.

And very often with the result of cure?—Yes.

Cross-examined by MR. COLERIDGE.

I will not ask you much, but if I understand you, you say that to

talk about anything like blood coming without a rupture would be an evidence of pure ignorance?—No; I did not say so.

I thought you said that it could not come without rupture of a blood-vessel, however small?—Yes; but I did not say what you have just said.

I though you said that it necessarily implied a rupture of a blood-vessel?—I say that it is physically impossible that blood corpuscles could escape from the vessels without a rupture.

If it be so, there could be no second opinion about it?—I am quite aware that the contrary statement has often been made; I am quite aware of that. You may be able to produce a passage from more than one author in which that assertion has been made; but it is really a matter of physical demonstration, scarcely a matter of opinion.

Is M. Lænnec a man of ability?—Certainly.

Attend to this:—

“Spitting of blood was attributed by the ancients to *rupture of the vessels of the lungs*; and this opinion, which is that of the vulgar, is perhaps still held by certain physicians who make a point of never admitting any new doctrines until they are so generally received as to claim their assent whether they will or no, and without examination. . . . It is not impossible that an aneurism of one of the branches of the pulmonary artery or a varix of the veins may, by rupture, give occasion to hæmorrhage, although no well-described instance of the kind has come to my knowledge However, it can no longer admit of question, in the present state of medical knowledge, that the greater number of cases of slight or moderate hæmoptysis consists in the simple *exhalation* of blood from the bronchial membrane.”

I do not want to go into the question whether you or M. Lænnec is right, but that is the opinion of an eminent man.—I have no doubt that in several books of authority you may find the statement made.

One is enough for me.—That is not a modern authority, because that passage was written many years ago, and probably M. Lænnec never looked through a microscope in his life. He was not a microscopic observer.

This is edited, I see, by Theophilus Herbert, with practical notes by Dr. Rammadge.—Modern microscopical observations show that the blood-vessels have no pores, and that the blood itself contains corpuscles.

However, persons might entertain that idea consistently with the opinion of eminent men?—Certainly.

I do not say whether it is right or wrong, but the opinion might be entertained?—Yes.

The Lord Chief Justice. When was that book published?—Fifty years ago.

Mr. Coleridge. This edition was published in 1846. It is a translation.

The Witness. Mr. Lænnec died long before that.

The Lord Chief Justice. They would be obliged to translate what he wrote.

Mr. Coleridge. I mean, my Lord, that it appears by the authority of Dr. Herbert and of Dr. Rammidge.

The Lord Chief Justice. On the other hand, if Dr. Johnson is right and the microscope has afforded means of ascertaining that the blood-vessels have no pores through which the blood could pass, and that the blood contains corpuscles which could not escape without an aperture through which they could pass, it follows that M. Lænnec is wrong.

Mr. Coleridge. I do not doubt it. I do not go into the question whether it is right or not. It is not for me to settle these things.

The Lord Chief Justice. I understand how you put it.

Now, the only object there could have been in introducing a question of this character was to impose upon the Court the idea that I had advanced a *false* and *unknown* doctrine, the Chief Justice immediately assumed *that* it must be false. I shall not dispute the question as to its truth or falsity. I believe what I wrote to be substantially correct. I do not believe that blood-vessels rupture every time a patient spits blood. You have above *Laennec's* opinion and now here is Sir Thomas Watson's:—

“When blood rushes out from internal parts through any of the natural apertures of the body, the person is said and supposed to have *broken a blood-vessel*. “But this is *rarely*, though it is sometimes, the case. In nine instances out of ten, if there be any rupture at all, it is rupture of the numerous capillaries only; “but even of this there is often no palpable evidence. Blood may *exude* abundantly from a surface which presents to the naked eye at least no appreciable “injury or change.”—p. 252.

“But as we are forbidden to speak of hæmorrhage by *exhalation*, we may suppose that in these passive hæmorrhages the capillary vessels have somehow become tender and fragile, so as to give way and spill their contents, &c.”

“A more probable hypothesis perhaps, is that which supposes some alteration in the condition and consistence of the blood itself; which thus becomes attenuated and “capable of passing through channels or orifices that healthy blood under ordinary “circumstances cannot penetrate, (pp. 256). . . . “This supposition is consonant “with the fact that hæmorrhages are known to occur where the blood is more thin and “pale, and serous than common,” (p. 257, Watson's Lectures).

So that even Sir Thomas has not been converted. His Lordship was *wiser* or more *foolish*.—Certainly he believed at once, that I at least must be wrong. Still I prefer to *doubt* with *Sir Thomas* !

Mr. Coleridge. Now, when were you first asked about this matter?—
I think about the month of March.

In the present year?—Yes.

You knew nothing of it before?—Nothing.

The Lord Chief Justice. Had you seen the advertisements before that?—I had seen the advertisements in the paper.

You knew nothing about the libel, and were not concerned in it?—
Not at all.

Re-examined by MR. KARSLAKE.

We did not go into this question about the exhalation of the blood, but as far as you know, has any modern writers entertained the view that M. Lænnec expresses in that work since the microscope has been brought into requisition?—No, I think not. I am quite sure that no microscopic observer could entertain such a notion. The corpuscles are of a size that could not possibly allow them to pass.

I have not attempted to follow this witness over the same ground taken by the other medical men. Every point in his evidence has been fully answered. He has written nothing on diseases of the chest. He is not in my opinion a reliable authority on this subject. He does not agree with Dr. Markham nor Dr. Odling that the blood would not take more oxygen than it requires. He leans rather to Cotton and Bennett who think it would. He does not agree with Dr. Williams in thinking the blood requires oxygen. He thinks he has cured a scrofulous ulcer (depending on a tuberculous state of the blood of the entire body) by a *local* application to the *uvule*! Sore throats, elongated uvulæ, enlarged tonsils, are in his opinion of no consequence; hence those who suffer from such affections would be great fools to be treated for them. Then he disagrees with Sir Thomas Watson in regard to the exudation of blood; and, lastly, he disagrees with everybody on the treatment of cholera!

DR. RICHARD QUAIN, *sworn.*

Examined by MR. FITZJAMES STEPHEN.

You are a physician of the University of London?—Yes; I am a doctor of medicine, and a Fellow of the College of Physicians.

You have read this work of Dr. Hunter's?—Yes; very carefully.

With respect to what has been said about the theory of consump-

tion which it lays down, do you agree, reading it as a medical man, with the last two witnesses?—I was not present when they were examined.

Then, perhaps you will state what theory you consider it to lay down with respect to consumption?—I really should be very unwilling to interpret Dr. Hunter's theory of consumption. I think that that is a very vague way to ask me the question. If you ask me a definite question, I will give you a definite answer; but I would not presume to interpret his theory.

I refer you to the passages about tubercle. Have you got a copy of the book?—I have got what is called the fifth edition.

Look at page 37, and tell me, do you recognise a passage commencing there, and running over to page 38, and to the bottom of page 39:—"I have told you that consumption is caused by tubercle," and so running on to the end of that letter? Have you read that passage?—Yes, I have; but it is a very long passage, which would require a very critical examination. If you choose, I will go through it. I see here a passage—"Softened tubercle is nothing more than carbon united with the elements of the tissues in which it was deposited."

What should you say of that?—I should say that it was an incorrect statement.

In what respect?—Tubercle is a great deal more than that.

What is it more than that?—It contains fatty matter in a distinct form. Carbon is one of the ultimate elements. For example, diamond is carbon, and sugar is nearly carbon; and, according to this definition, tubercle may be either diamond or sugar.

The Lord Chief Justice. Does tubercle contain carbon as other animal matters contain it?—Quite so.

I put this for my own information. You would not find, if you took a tubercle and examined it ever so closely, actual carbon in it; but you must decompose it before you get the carbon?—Yes; you must resolve it into its ultimate elements, and then you will find less carbon in it than in the healthy tissues of the body.

This statement I have already shown to be utterly opposed to fact; but, then, remembering as I do Dr. Quain's little mistake about the "hypophosphites," and the "hypophosphates" which a short time ago caused considerable amusement to his medical friends, I am not surprised that he and I do not quite agree.

You would decompose it as you would any other matter?—Yes; as you would sugar, or anything else. But I would wish to say, that there is less carbon in it in proportion to the other constituents than in the healthy tissues of the body.

Mr. Stephen. Now I will take you to a passage at page 90, in which Dr. Hunter says—"What does chemistry tell us of the essential nature

of tubercle? The analysis of Scherer proves that 54 per cent. of it is pure carbon, and the remaining 46 per cent. made up of the elements of disintegrated tissues of the lungs in which it is deposited." Are you acquainted with Scherer's analysis?—I have a copy of it here. In Simon's "Animal Chemistry," which is regarded as a standard work, that point is distinctly stated.

Do you consider that Dr. Hunter's statement, as to what Scherer's analysis is, is a fair representation of Scherer's analysis?—It is a statement of the fact, but its meaning is misinterpreted.

Will you explain how it is a misinterpretation of the meaning?—Scherer states that "crude pulmonary tubercles yield a little fat *

* * * * *

tubercle may be regarded as protein"—protein is the basis of the healthy textures—"from which five times * * *

* * * * *

has been removed." That is to say, that this tubercle, which is said to consist essentially of carbon, contains less carbon than the healthy textures of the body. Therefore, to interpret that as meaning to say that carbon is the source of tubercle, and the essential and principal source of it, is a misinterpretation of the views of Scherer.

After that, do you agree with this description of tubercles, or their origin, in page 38—"Tubercles result from imperfect decarbonization of the blood. Whatever prevents the free admission of pure air to the air-cells will produce them"?—Oh, certainly not at all. It is not at all correct.

Now, assume that Dr. Hunter's theory, or part of his theory, is, that tubercle is a deposit of carbon in the lungs, and that he has provided means for introducing oxygen, and bringing it into contact with that carbon, and so decomposing the carbon—do you consider that that operation would be possible?—If Dr. Hunter had seen tubercle deposited in the lungs, and their effects, and had witnessed the means or possibility of introducing agents that would act upon the lungs, he would never have made that statement, believing it to be true.

Besides, assuming, for the sake of argument, that a portion of carbon was deposited in the lung, a portion of pure carbon, and that by some means or other you could bring oxygen into contact with it, would the effect of that oxygen be to decompose the carbon?—If it were set on fire it would—not otherwise.

That would not be a wholesome process for the lungs, I presume?—I presume not.

The Lord Chief Justice. You do not think that the mere animal heat of the body would be sufficient?—Not upon a solid substance, and tubercle is a solid substance.

Not *very* solid, is it, Dr. Quain? Crude tubercle will grease

the fingers, and break down into a soft pulp—then it is often like cream in the third stage of consumption.

Mr. Stephen. Do you agree that catarrh is the first step towards consumption?—A person may have catarrh and ultimately consumption, but there is no necessary relation between the two.

Is it a usual thing for a neglected cold to advance to consumption?—It is not at all an unusual thing.

Cross-examined by Mr. COLERIDGE.

I will only ask you, Dr. Quain, this question. I presume you heard nothing about this libel until long after it was written?—I read the very first letters that appeared, and I was very much surprised—I was quite astonished.

Here we find the same playing upon words. First Dr. Williams likened carbon to charcoal, now Dr. Quain talks of *sugar*. As the former built his evidence on charcoal, so the latter proposes to do so on sugar. Considering the great experience Dr. Quain must have obtained with that chemist at Limerick, with whom he served his apprenticeship, I feel disposed to bow deferentially to his opinion. Chemists' boys are apt to grow up sharp. From sugar he falls back to charcoal when the Chief Justice enquired whether oxygen would combine with tubercle supposing it to be carbon. Now if his Lordship had asked him if it would combine with hydro-carbon he would have been compelled to say yes. Was it not very highminded and highly honourable in Dr. Quain, to mislead the Judge and the Jury by a quibble which would not have been attempted but for their known ignorance of the distinction between *carbon* and *hydrocarbon*—between an *inorganic body* and an *animal substance*?

We have now arrived at a very important stage in the investigation. The "*chemist*" is about to take the stand to clench by his chemical knowledge all that had been previously said by the other *scientific* witnesses for the defence. They were one and all rather deficient in *chemical* experience, and were, moreover, so *uncertain* about the composition of everything—Dr. Williams having even forgotten that of the blood—that I did not *fear* them; but here the case is different. I

hope I enter upon an analysis of Dr. Odling's evidence with a proper sense of my own unworthiness and of the great perils of the undertaking. These chemists can prove anything, if we only admit the truth of their premises; but this it is never wise to do until after they have *proved* them to be correct. When I look at the wide differences and discrepancies which exists between the results attained by *great* chemists, I am always naturally suspicious of *little* ones, especially when they have anything to gain by confounding me with improbable if not impossible statements.

Dr. Hughes Bennett, the distinguished Professor of the Institutes of Medicine to the University of Edinburgh, in speaking of what *some* chemists have found in tubercle and others not, says,

"Some chemists have also detected caseine, the existence of which is *probable*; "others gelatine, the presence of which is more *doubtful*. The statement of Gatterboeck, that it contains a peculiar animal matter (why matter?) has *not been confirmed by other analysts*."—p. 26.

so that it is clear that they do detect in some instances what *does not exist*, and in the other what is *doubtful*.

Here we have a striking proof of the "glorious uncertainty" of chemistry.

"Lavoisier and Seguin estimate the quantity of carbonic acid given off by the lungs of an adult man in 24 hours, at 14,930 cubic inches; Davy, at 31,680; Allen and Pepys, at 39,600 the estimate of the latter (Pickford's "Chemistry of Respiration," p. 151) being nearly three times as much as the former.

Now, take another illustration, the quantity of aqueous vapour exhaled from the lungs has been calculated by various chemists as follows:—

Menzies calculated it at	6 ounces in 24 hours.		
Abernethy	9	"	"
Thompson	19	"	"
Hales	20	"	"
Lavoisier	28	"	"

(Pickford, p. 259).

The last it will be seen is just four and a half times as much as the first. These facts show us clearly that chemistry is safer when taken *cum grano salis*."

DR. WILLIAM ODLING *sworn*.

Examined by Mr. KARSLAKE.

You are a Fellow of the Royal Society, a Fellow of the Royal College of Physicians, and Lecturer on Chemistry at St. Bartholomew's Hospital?—Yes.

I need hardly ask you whether chemistry has been your great study?—It has.

I do not know whether you have here Scherer's Analysis of Tubercle, but you agree entirely with what has been said on the subject, I presume?—I disagree with the observation that Dr. Quain made just now. The statement in Dr. Hunter's book is not merely a misinterpretation; it is a misstatement of Scherer's analysis.

This is not a kindly beginning. Dr. Quain, he says, misinterprets me, and I misinterpret Scherer! Now, it is quite possible, for *interpretation* depends on judgment; and I have had so many illustrations in the misinterpretation of my own book by the medical witnesses for the defence, that I have grown diffident. They will no doubt say that they did so *ignorantly*—that is, without *design*. If I have misinterpreted Scherer, it was *certainly* without design. Of course, Dr. Odling is himself free from the possibility of any fallacy of judgment. We shall be better able to decide upon this after we have proceeded a little further.

Does Scherer's analysis teach anything at all about the essential nature of tubercle?—It shows that tubercle has the same ultimate composition as nearly all the tissues of the body, healthy or diseased; the same as a beefsteak, in fact.

The Lord Chief Justice. There is some slight difference?—The books say that it contains rather less carbon, and on general principles the probability is that it would do so; but when you put it in practice the difference is so slight, that I should not pay much attention to it.

Mr. Karslake. At all events, tubercle is far from containing more carbon; it contains, if anything, less than the rest of the body?—If anything.

The muscles contain the same quantity of carbon?—Very much the same.

Regarding the analysis of tubercle and muscle being identical, I fancy he is not quite correct. The following is the elementary analysis of muscle from Müller's "Physiology:"—

	Carbon.	
Muscle	48.30	
Tubercle	53.50	Scherer.
Tubercle	56.00	Glover.

Here we see there is a *marked difference* in the quantity of carbon which enters into their composition. I have no doubt Mr. Odling could diminish this difference if I would accept *his* analysis; but I prefer *standard* authorities.

I have elsewhere shown that tubercle contains even more carbon than the *impure venous blood*, and that, mingled with what I regard as the essential element of tubercle, there is fibrin of low vitality, and albumen undergoing transformation into that hydro-carbonaceous compound which constitutes softened tubercle; all of which enter into the analysis, and assimilate it to that of the tissues. It is only as the *germ* or *nucleus* that I regard the hydrocarbon as the essential element of tubercle.

Fat contains considerably more carbon?—Fat considerably more.
About 72 per cent?—Yes; fat contains much more carbon.

Yes, the *compound* called *tubercle* contains less carbon than fat, but then I contend that the essential element of tubercle is fat—that is both effete and worn out fat—that has lost the characteristics of healthy adipose matter, and become a product fit for excretion. That the excess of this in the system is the direct cause of tubercle, and a result arising from defective excretion by the lungs. Fat is essentially *hydrocarbon*.

The Lord Chief Justice. Let me ask you one question upon this. This proportion of carbon, this composition of the tubercle, which you say differs very little from the other tissue, is that matter of general professional knowledge?—It is a matter stated in all the text books of physiological chemistry or animal chemistry.

Mr. Karlake. Especially in Scherer's analysis?—Scherer's book is not much known, but Scherer's analyses are very well known.

And adopted by other writers?—And adopted by other writers.

I have elsewhere given the analysis of the arterial and venous blood, of fibrin and of albumen, and shown that in each case the quantity of carbon is less than in tubercle; so

that his evidence on this point is not correct, or, if so, it is contrary to that of the eminent chemists quoted by me.

Have you experimented on the quantity of atmospheric air breathed by a healthy subject in ordinary respiration?—No; but it is a matter of ascertained knowledge.

What per centage of oxygen does the atmospheric air contain?—Rather more than 20 per cent—one-fifth.

It is a pity that these chemists will not be more exact. Here he is at the outset ignorant of the components of the atmospheric air. The following, from Pickford's "Chemistry of Respiration," will show his error on this point:—

Oxygen	19·7	in 100 parts.
Nitrogen	78·8	„ „
Aqueous vapour	1·4	„ „
Carbonic acid	·1	„ „
<hr/>		
	100	„ „

Take an average subject, how much of that atmospheric air is expired by a person every minute?—The best calculations give rather more than 400 cubic inches of air, or eighty cubic inches of oxygen per minute.

Every minute?—Yes.

That is equal to 4,800 cubic inches in the hour?—Yes; it is equal to nearly three cubic feet per hour of oxygen.

Does the quantity inspired vary with the rapidity and depth of respiration?—Yes; in a deep inspiration the quantity of air taken in may be increased tenfold, and doubling the average amount of inspirations in a minute, you would double the quantity of oxygen.

So that that quick breathing of the asthmatic person is for the purpose of inhaling a greater quantity of air than he would do by ordinary respiration?—No doubt.

Now, supposing that oxygen can be inhaled, taking the inhalation to last from ten minutes to a quarter-of-an-hour three or four times a day, would the oxygen inhaled under these circumstances be at all considerable?—The inspiration of four or five gallons more or less of oxygen in a day, is a quantity that is perfectly insignificant in proportion to the total quantity inspired in a day. I say that, because a bladder will hold a gallon or a gallon and a half, and the inhalation of about four or five gallons is not quite one per cent.

When you say that, you are speaking of pure oxygen?—I am speaking of pure oxygen.

Of course if that is mingled with other vapours or gases, there

would be a considerable diminution in the quantity?—It would be still more insignificant.

A far better description than he gave on this point can be obtained in any elementary work on the function of respiration. It had no application to the case, since, as it is the last straw that breaks the camel's back, so it is the "*insignificant*" quantity which makes up the vital deficiency of the respired air, and increases its potency. The presence or want of ozone in the air is supposed to be all the difference between a healthy and a pestilential atmosphere; the poison of cholera and the plague have never yet been detected by all the efforts of chemical art, though known to exist on every hand. Then, too, look at typhus fever wards, so fatal, that they are only entered at the peril of life—an atmosphere reeking with poison. What has chemistry done to clear up these points? There are scarcely two analyses of anything which agree.

Now if the addition of a little more oxygen is of such small consequence, why does Dr. Williams try to introduce oxygen into the blood by giving what he calls agents "*which contain much oxygen in loose combination.*" From fifteen to twenty drops of dilute nitric acid, or two or three drops of the strong acid given three times a day, would not yield much oxygen. Dr. Odling can no doubt tell us how many cubic inches, but certainly it is something "*very insignificant*," and yet Dr. Williams thinks this *insignificant* quantity does good.

But is the quantity got by inhaling so small as Dr. Olding supposes? The whole blood of the body is acted upon in two minutes by whatever we inhale. In ten minutes the whole blood can be rendered chemically pure, and an impulse given to the system which I know from experience to be productive of vast good.

Is chloric acid, which we have heard mentioned, known generally in the Pharmacopœia?—It is not in the Pharmacopœia.

What has chloric acid hitherto usually been employed in?—I have never heard of it being used for anything but in the manufacture of a substance for making fireworks.

The Lord Chief Justice. You never heard of it being used medically?—Not to my knowledge. If it be used at all, it is used in secret in this country.

Mr. Karlake. Is it highly explosive unless considerably diluted?—Chloric acid is only known in the form of a stronger or weaker solution, and the stronger solution is extremely inflammatory. It inflames and chars substances. It sets fire to paper immediately it touches it.

The Lord Chief Justice. What with regard to the weaker solution?—The weaker solution is more stable.

You can reduce the strength by the addition of water to any degree?—Yes, to any degree.

Mr. Karlake. Supposing you do reduce it, does it give off oxygen in a pure state?—If you take the strong acid and heat it nearly to boiling point, it gives off both oxygen and chlorine gas, in the proportion of three volumes of oxygen to two volumes of chlorine.

The Lord Chief Justice. The stronger solution subjected to hot water, do you mean?—To ebullition. The substance itself boils at a temperature above boiling water; so I am speaking of the boiling of the substance.

It gives off what?—Oxygen and chlorine gases, in the proportion of three volumes of oxygen to two volumes of chlorine.

Mr. Karlake. May I take it that in inhaling that oxygen you must necessarily also inhale the chlorine?—Certainly.

What is the effect of the chlorine in that state?—Chlorine is an extremely irritating gas, even when so much diluted as to form only half per cent.

The Lord Chief Justice. Half per cent. of what?—Half per cent. of the air inspired.

What would be the effect if the chlorine were reduced, and if you got a weaker solution?—If you get the dilution much below half per cent., it may be breathed and has been breathed.

Mr. Karlake. But even in that state, must it be breathed altogether—oxygen and chlorine?—It comes to this, that if you have the oxygen in any reasonable proportion you have the chlorine so strong as to be irritant, and if you dilute the chlorine so as to make it not irritant, you proportionably dilute the oxygen.

Dr. Olding may be a judge of “*fireworks* ;” I am not. It is possible that his experience in their manufacture may enable him to know what combustible materials are employed in inflating fire-balloons to make children wonder and fools gape. On these points he ought to be a better judge than I am.

Regarding the “*irritation*,” that is not true in any sense whatever; but since he never used chloric acid or knew it to be used medicinally, whatever he may have done in fireworks, his opinion on this point is not worth repeating or refuting.

The Lord Chief Justice. If you were to dilute chloric acid, so as that the amount of chlorine gas given off upon boiling were reduced below

that half per cent. of the atmosphere?—Then the oxygen would leave about three-fourths per cent. additional.

Mr. Karlake. Now, supposing you reduced it to that extent, could you get more oxygen from the inspiration of the chloric acid heated than you do from the atmospheric air, or to any considerable extent more?—It is a matter of exact knowledge that eighty-five grains of chloric acid—absolute chloric acid I mean, which would probably be about a drachm—a drachm of water being sixty grains, and this being heavier, would give off thirty-five and a half cubic inches of oxygen, eighty being absorbed in a minute habitually.

That is the strong chloric acid?—The absolute acid.

Can you form an opinion from that, what quantity of oxygen would be given off by chloric acid in such a form as that it could be inhaled by a patient; would it be more or less considerable?—Of course, drachm for drachm the total quantity of oxygen that would be given off would be less in proportion to its dilution, but dilute acid only gives off its oxygen and chlorine very slowly and imperfectly.

But supposing you dilute it to such an extent as to make it capable of being breathed by a patient, would the oxygen given off in combination with the chlorine be more or less than the oxygen of the atmospheric air?—It would be quite insignificant.

Did you hear from Dr. Hunter or any other witness, anything else that produced oxygen?—I should like to say before going into that question, that if there were tinctures present, there would not be a particle of oxygen given off.

How is that?—Because alcohol absorbs the oxygen immediately from the chloric acid.

The Lord Chief Justice. You say that alcohol absorbs oxygen from chloric acid?—Yes.

Is that peculiar to chloric acid, or is it applicable to any form in which oxygen and alcohol meet?—Not any form in which they meet; it is peculiar to chloric acids and other acids of the same character—permanganic acid, nitric acid is the same; but the observation does not apply to sulphuric acid. It applies to chloric acid undoubtedly and immediately.

This statement I have tested with chloric acid, and alcoholic tincture of the strength employed by me, and I know that it is utterly without foundation in fact. Not even the odour of chlorine was perceptible. But this is no test, since chloric acid is not employed by me to generate *free* oxygen, but as one of several *oxydizing* agents.

Mr. Karlake. Supposing the chloric acid was used in combination with stramonium, would that absorb the oxygen?—The oxygen of chloric acid is absorbed by all organic matters with greater or less rapidity. Therefore it is absorbed by stramonium and conium, and

all these bodies, with what rapidity I cannot say; but with the tincture of either it would be absorbed instantaneously.

Then the patient who inhaled would get no oxygen, but he would get chlorine?—The chlorine would be absorbed too.

Now, with regard to these nitre pastilles, do they evolve any oxygen in burning?—None.

The Lord Chief Justice. What do they consist of?—Charcoal and nitre, and generally some vegetable substance, stramonium or gum benzoïn, or some other aromatic substance.

Here the chemist is scarcely honest, since, when he said that the decomposition of nitrate of potassa gave no oxygen, he must have known that he would be taken *literally*, and that the jury would suppose that the gas liberated would not *oxidize* the blood—would not give additional oxygen to the blood. It was a mere quibble over *oxygen combined* with nitrogen; and *oxygen uncombined* with nitrogen.

Then he assumes that my pastilles contain a variety of substances which they do not contain. Every word he uttered on that point, therefore, was based not on fact but on *theory* and *assumption*.

Mr. Karslake. Is chloric acid volatile, or capable of being inhaled?—Not as chloric acid.

But mixed with hot or boiling water?—If chloric acid, that is, strong chloric acid, be boiled, it undergoes decomposition, and its products of decomposition are vapours. The chloric acid itself is not vaporous.

As regards carbon being the base of tubercle, you have already given your evidence?—Carbon is only the base of tubercle in the same sense that carbon is the base of all other tissues whatever.

Now as to carbon in the blood being inimical to life, do you know as a scientific man whether the blood of consumptive patients contains more carbon than in health, or is it the other way?—The difference is not very striking; but so far as it goes, the blood in consumption contains less carbon and more oxygen than the blood in health.

Here are several assumptions for which he can have no authority whatever. There are no known facts on which he can base his assertion that the "*blood contains more oxygen*" than in health. Every man's common sense rejects it at once. You might as well say that a *starving* man had too much food. The oxygen we receive is measured by the quantity of the air we inhale at each breath, and as that is lessened the oxygen supplied to the blood is correspondingly lessened. The craving

for air is as much a proof that the oxygen is deficient as *thirst* or hunger are proofs of a want of food and water. There are no elementary analyses of the tuberculous blood on which Dr. Odling could base such an absurd statement. I dispute its truth and shall be glad to see the authority upon which he makes it.

Dyspnœa, or want of breath, is the characteristic *symptom* of all forms of disease of the lungs, and it is an evidence in the opinion of all *really* scientific physicians, that the oxygen in the blood is less than is required for health.

Sir Thomas Watson says—

“Now, upon what does the symptom [*dyspnœa*] depend? It may ultimately be “referred to an altered proportion between the quantity of atmospheric air [oxygen] “that reaches the lungs, and the quantity of blood that is sent into them from the “right side of the heart to be converted from *venous* to *arterial*. This doubtless “is at the bottom of almost every case of *dyspnœa*.”—p. 3, vol. ii.

Again, speaking of the causes which produces it—

“They all operate ultimately by destroying the just equilibrium between the “*blood* and *air* which meet to undergo mutual and chemical change in the lungs. “. . . . If the access of air be still impeded, blood but half decarbonized begins “to circulate through the arteries and to linger and *stagnate* in the lungs. “The quantity of blood being the *same*, but the *air* inspired *too little*, there will “be *dyspnœa*.”—p. 4, vol. ii.

Now, as every patient suffering from consumption has “*dyspnœa*,” according to Dr. Watson, his blood is *not* properly oxidized, and hence, circulates “*but half decarbonized*.” This is *true* science. The *spurious* science of Dr. Odling says, Nature is a fool, there is more oxygen in the blood than in health. Again I say, *I* prefer Watson to Odling—practical authorities to chemical *theorists*!

The difference is small, but that is so?—That is so. I should like to say with regard to that expression of carbon being inimical to life, that two distinct things are there jumbled up together. Carbon which has not been used is not in any sense inimical to life; it is an essential to life, like coals are to a steam-engine; but carbon which has been used is inimical to life.

The Lord Chief Justice. You mean carbon which has passed through the system, and has become worn out?—Combined with about three times its weight of oxygen, when it may be termed oxydized carbon, it is inimical to life, like the smoke of a steam-engine, but then it is chiefly in the form of carbonic acid, and it could no more make tubercle than it could make flesh and blood.

Unused carbon was not what was meant by me, and no person not resolved beforehand to put a false construction upon a very simple matter could have *pretended* to suppose so. Effete organic hydrocarbon is a very different thing. Every work on organic chemistry of which I have any knowledge speaks of the elimination of carbon as I speak of it. For example, does Liebig, in the following paragraph mean “*charcoal*?”

“When the amount of elements of respiration of fat, sugar, or starch in the food is increased without an increase of the oxygen necessary for their consumption in the body, then the amount of *carbon* and hydrogen in the *fæces* of those in whom the conditions of the formation or deposition of fat are wanting, *mus*, increase in an equal proportion.”

Liebig here uses the term as I use it, and no honest, well-meaning, conscientious, and well-informed chemist would pretend to put a false construction upon *his* words. It was presuming upon the ignorance of the Court if done wilfully. It was *ignorance* if done honestly. To my mind there is nothing in nature more elastic than indiarubber, unless it be the consciences of the so-called *scientific* witnesses for the defence in “*Hunter v. Sharpe*.”

Mr. Karlake. Is it correct in your judgment to say that oxygen inspired will combine with and remove tubercles deposited in the lung?—There is not the slight reason to believe that the contact of oxygen with tubercle will destroy that tubercle, any more than there is to believe that the contact of oxygen with the lung would destroy the lung.

This is chemically false, and shows an entire ignorance both of the *nature* of tubercle and of the natural resistance of *vital structures*. The body resists the action of decay or decomposition in a direct ratio to its vitality. If its vitality be *high* it has a much greater power of resistance than if it be *low*. Now the tissue of the lung has a very high and active vitality; on the other hand, if tubercle has any vitality, which is disputed by many pathologists, its organization is of the feeblest character. To say therefore that the oxygen would be as likely to destroy the lung as the tubercle, shows an unacquaintance with well-known facts.

Is it the fact, according to your experience, that the discharge of carbon from the body is increased by breathing air with an excess of oxygen?—Some very accurate experiments were made by Reamur, in Paris, with atmospheres containing double and treble the normal proportion of oxygen, and the discharge of carbon from the body was not altered.

When were those experiments made?—I think in 1849.

The Lord Chief Justice. Do you calculate from that that a given quantity of oxygen being required to carry off the worn-out carbonaceous matter of the system, that with an increased bulk of oxygen you get no more carbon off?—Quite so.

Here are two statements, which require to be received *cum grano salis*.

“Demarquay and Leconte have shown that they as well as their friends could “readily inhale from twenty to thirty litres without suffering any injury, and “Ducroy has stated to the Academy of Paris, that pure oxygen, contrary to the “general belief can be inhaled for several hours without being detrimental to “health ; that its action is antagonistic to that of chloroform ; that it is a powerful remedy for the disagreeable accidents arising from chloroform and other “anæsthetics ; and that in asphyxia from poisonous gases—as, for instance, carbonic acid—it exercises likewise a beneficial influence. Ducroy therefore proposed to make every patient awaking from a chloroform-narcosis inhale oxygen in “order to rid him of headache and other inconveniences following the administration of that anæsthetic.” (Beigel, p. 72.)

Here we have proof that the inhalation of oxygen is not followed by any “*excitement*,” and that it will act upon the system most powerfully and beneficially. These writers had no object to misrepresent facts. They were not *instruments of a conspiracy* to promulgate error and suppress truth. Now, Dr. Odling must, if not grossly ignorant of medical literature, have known that these views of Demarquay, Leconte, and Ducroy had been published, and that they proved the very reverse of his theory, and justified all that I claimed for oxygen ; he must further have known that Erichsen found that he could restore suspended animation by inflating the lungs with oxygen when he could not do so by inflating them with atmospheric air. He knew that his view was not generally accepted, and that at best the point was a *moot* point ; and yet, knowing all this, he allowed the judge and jury to believe that his theories were established and mine utterly unsupported by authority, while the very reverse was the fact.

But then taking the converse, supposing you have too little oxygen, you get a redundancy of carbonaceous matter?—I was going to say that of the entire quantity of oxygen habitually taken into the lungs only one-sixth is used in the system, so that there is habitually an excess of five-sixths.

In the system?—Taken in by inspiration.

Do you say that because so much comes back again?—Yes.

Does it not come back in combination—does not that which you take by inspiration come back combined with the worn-out carbonaceous matter existing, and in the shape of carbonic acid?—Some of it does, but there is always a large excess. Of the entire quantity of oxygen taken in by the lungs only one-sixth is used up in the formation of carbonic acid; the remaining five-sixths are discharged as oxygen.

Here again "*the chemist*" is entirely at fault. He says that if you take in an excess of oxygen, "*you get a redundancy of carbonaceous matter*"—passing the fact that he has only just told us we would get no action whatever from the increase of oxygen—the present statement is based on the idea that oxygen is only useful in manufacturing carbonic acid, which is a pure delusion. It is the source of every vital action in the body, and it only combines with and eliminates the waste or worn-out carbonaceous matter. It could not make carbonic acid to a greater extent than there was *effete* carbon. It is, too, an admission of that as a *fact* which on the authority of the experiments of Reaumur he has just told us was *not* a fact, viz., that more carbonic acid was exhaled. It is a mere *chemical sophism* which reflects little credit on either his *chemical* or his *physiological* education.

Then, on the second point, he asserts that the oxygen of the air inspired comes back as carbonic acid, having combined in the lungs with the carbon of the blood (!) It does no such thing, in the opinion of any *reliable* physiologist. It is directly absorbed into the blood, and carried throughout the system. The carbonic acid which comes back, existed, *ready formed as carbonic acid in the blood*, and is merely thrown out in passing through the lungs. The blood exchanges carbonic acid with the air for oxygen, volume for volume; but the oxygen does *not* unite in the lungs with the carbon of the blood, and is *not* returned again at that time.

But this is not the only error of his statement. Not only is he in error as to one-sixth of the oxygen being used up in the formation of carbonic acid, but in what is expelled from the lungs by the act of expiration. He says, "*the remaining FIVE-SIXTHS are discharged as oxygen!*" Now the truth is, *no oxygen* is so discharged. What is discharged is the unconsumed *atmospheric air*, only *one-fifth* of which is oxygen, and mingled with it we have *ammonia, watery vapour, and carbonic acid*.

Mr. Karlake. So that nature provides five-sixths more air than is absolutely necessary for the purpose of discharging the carbonic acid; is that so?—I will put it in this way. It is found by experiment that if you put a person in a room absolutely closed, he would spoil the air in two ways: by absorbing the oxygen and discharging carbonic acid would be one way; but if you absorb by chemical means the carbonic acid as soon as formed, you might reduce the oxygen in the chamber to a great extent, and the person would suffer no inconvenience. It is the presence of the carbonic acid which makes the air unwholesome, rather than the decrease of oxygen. The one acts with greater intensity than the other, so that the person would become poisoned by his own carbonic acid long before he felt the loss of oxygen.

Then, of course, *small* lungs are as good as large ones, because, though we only receive half the quantity of air, we can just take two-sixths at each inspiration instead of one-sixth. If his argument means anything it means this. Does not every man living know that it is a fallacy? Why have men with *large* lungs and great breadth of chest, strong muscles and a vigorous constitution? Why do men, the function of whose lungs has become impaired, emaciate and grow feeble, but because the quantity of blood becomes reduced to the standard that the smaller volume of air will purify; because the assimilation of chyle in the lungs takes place only in proportion to the volume of air, and because as the volume is lessened so is the quantity of nutriment which can be built up *diminished*. Now, clearly, if the blood only takes "*one-sixth*" of the oxygen from the air in health, and could take *one-third* or *one-half* from any given volume if it required it, the same amount of chyle should be built up, and the same strength of muscle maintained, which is not true. If it were true it

would really be of little consequence whether a man had one lung or two. We might fill the air-tubes of one with cod-liver oil enough to last a week, and thus economise food and fuel! I am sure nobody *but* a chemist who knew more about fire-works than physic would commit himself to such senseless vagaries.

Then there is another error in the view expressed by Dr. Hunter when he says, "The fumes of burning charcoal, when diffused through the air of a room, cause death, in the same way as drowning, by simply preventing the oxygen of the air from reaching the lungs."—Well, no. The fumes of burning charcoal will cause death by something given off from the charcoal: not purely carbonic acid, but a much more poisonous emanation—carbonic oxide.

On my theory it should be much more poisonous, for it contains *more* carbon and *less* oxygen in a given volume. But here again he is off in the clouds of theoretical speculation. Nobody knows whether the carbonic oxide acts primarily on the air or on the blood. It may, for anything we can know, first devour the oxygen of the air, rendering it irrespirable, in which case death would take place by *asphyxia*, as in drowning. But is it not absurd to say that I should have discussed all these abstractions in a *popular* treatise on medicine.

Chlorate of potash was mentioned as one of the substances said to evolve oxygen when burned as a pastille. Do you know the effect of chlorate of potash in a pastille?—I have never seen chlorate of potash used in making a pastille, but I have seen it in action with carbon. It gives no oxygen at all, because the oxygen is absorbed by the charcoal by its burning.

If you set it alight in a pastille, you mean?—I know that you get no oxygen from nitre pastilles, and I have no doubt that it would be the same with chlorate pastilles.

The Lord Chief Justice. Do I understand you to say that supposing the respiratory organs to be affected by chronic inflammation, so that the passage of the air into the lungs is obstructed, still nature has so provided that there shall be an excess of oxygen taken in by the inspiration, notwithstanding the obstruction, and that there will be still enough to supply the wants of the system?—That is so; and I put it in another way. Supposing you had lungs, one-fourth of which were rendered useless by tubercle, and you added more oxygen to the air, then each inspiration would have the same chance of getting oxygen as if the lungs were healthy under ordinary circumstances; but that oxygen would make more carbonic acid, and you would have more

carbonic acid formed than the lung could get rid of, whereby the carbonic acid would remain in the blood.

Therefore you would be doing no good?—No; you would be bringing more smoke where there was a narrower funnel for the smoke to come through.

It is humiliating to our profession that such arrant nonsense as this should pass as science. First, the chemist assumes that carbonic acid would be formed in proportion to the oxygen received, when he must or ought to have known that the blood gives up the same volume of carbonic acid that it receives of oxygen, and hence that it could not take more oxygen than it could give up carbon. The funnel is *not* “*narrower*” than in health, and we have no reason to believe that any limit has been fixed to the amount of carbonic acid that can be expelled. If we inhale *more* oxygen than there is carbonic acid in the blood, the blood does not receive it. If we inhale *less*, a portion of the carbonic acid must remain in the blood, poisoning the source of life. Hence it is that I use the expression, “Carbon being a poison most inimical to life,” &c.; and hence, also, Dr. Carpenter, in his “Physiology,” says:—

“Of all these injurious ingredients carbonic acid is without doubt the one most abundantly introduced into the nutritive fluid, and it is also *most deleterious* in its effects on the system if allowed to *accumulate*.” (“Physiology,” p. 263.)

Now, these scientific witnesses have taken advantage of the fact, that in my book, written for popular reading almost exclusively, I did not particularize the carbonaceous compounds meant, but used, for simplicity, the general term, carbon. If I had done differently, then I should have been drawn into the use of such terms as carbonic acid, carbonic oxide, hydrocarbon, cholesterine, and others similar; all of which compounds owe their properties to the element carbon. This would have confused and rendered the book valueless to the public. This is the extent of my offending, and it is on this basis that these medical men banded themselves together, and went into the witness-box to try to blast my reputation. All their evidence was built on this foundation.

Mr. Karslake. Have you studied the question of the practicability of administering pure oxygen?—Pure oxygen may be administered.

As a chemist I do not ask you what the effect might be; but sup-

posing it to be essential to administer pure oxygen, what is the mode by which it can be administered?—In a rough sort of way in a bladder, but most efficiently in a gas-holder, or as in Reamur's experiments in a chamber.

How is the chamber prepared?—I have not seen Reamur's chamber but I have seen Petticoffer's chamber at Munich. It is a room about twice the size of that jury-box, in which all the air that goes in is analysed, and all the air that comes out is analysed, and then they make artificial atmospheres and try their effect.

Using these mixtures of chloric acid with stramonium, conium, and so on, in your opinion would any oxygen be inhaled?—None.

So far as chlorine is inhaled, is it an irritant rather than a sedative, or a useful medicine?—Unless very diluted it is an irritant.

Cross-examined by Mr. COLERIDGE.

You have not seen one of Dr. Hunter's pastilles?—No.

Do you say that chlorate of potash does not disengage oxygen?—No; it is the agent most frequently employed for it, but it requires a heat approaching to redness to give it off.

Then it does give off oxygen?—Yes.

Although he never saw one of my pastilles and knows nothing whatever in regard to them, further than that they sometimes contain nitrate of potassa, and at others chlorate of potassa, still he swears that they will not give off oxygen! Now, he admits that chlorate of potash will give off oxygen if heated to redness, and that it is the *very* agent chemists employ when they want to get *pure* oxygen! Every patient I have had knows that the pastilles are not only heated to redness, but that the metallic case is melted in the combustion. Still he says it won't give off oxygen because "*the charcoal will absorb it.*" But I *don't* use any charcoal! Now, this statement of the chemist having gone abroad, I am willing to submit the matter to the test. Let the chemist put up the expenses of the action against the *Pall Mall Gazette*, and make the more dependent on my producing oxygen, which shall perform all the functions of oxygen gas, supporting combustion, &c., from one of my pastilles made with chlorate of potassa! If he does not do this, I shall be justified in handing him down to posterity as a chemical windbag *collapsed*.

You say that would be the case with chloric acid if boiled?—With this difference, that chlorate of potash gives off only oxygen, and

chloric acid gives off a mixture of two parts of oxygen and three of chlorine.

The Lord Chief Justice. You say that all the oxygen that goes into the lungs does not come out?—It is shown in this way. If you measure the quantity of air taken into the lungs, of that air five-sixths of the oxygen is given back unused.

So that there is always more supplied than is used?—Yes.

Is that the case always? I mean, for instance, do you mean to say that there is always that excess? There must be some difference in the atmospheres that people breathe?—A very great difference, but that difference depends more on the presence of substantive impurities than upon the absence of oxygen.

If this were *true*, no person could feel *shortness of breath*, or *want of air*, when they were in a pure atmosphere, which every *patient* suffering from consumption, bronchitis, or asthma, knows is absurdly incorrect. I do not believe there is one physician living who could honestly say he believed this doctrine of the chemist. No one *could* feel shortness of breath until more than *five-sixths* of his lungs were destroyed, for if he only requires in health *one-sixth* of the oxygen of the air, and could take more if he required it, even to the whole, why, he would merely draw upon the excess, and no shortness of breath would be possible until after his disease had destroyed more than five-sixths of the lungs. If this be true then is there nothing false?

Is that so?—That is so.

For instance, in this court it is not the absence of oxygen, but the presence of something else, that makes it unpleasant at five o'clock?—Yes, it is the presence of the emanations from the breath.

The carbonic acid gas being heavier than the air would naturally fall to the floor, but even were this otherwise it ought not to be hurtful if the blood has “too much oxygen,” and Dr. Williams has for thirty years made his consumptive patients inhale their *carbonic acid* over again. It ought to have been a good thing, as it was opposed to my theory, and harmonized with Dr. Williams’s *practice*. I think this witness should not have made such an admission. It decidedly militates against both Dr. Williams and himself.

You say that pure oxygen may be inhaled?—Yes.

Supposing there were carbon present in the lungs bodily would

oxygen decompose it at the temperature of the lungs?—Certainly not. It could not.

This refers to “the coals” he spoke of a little time back. He was thinking only of *inorganic carbon*, and his answer therefore was irrelevant. It means simply this—Supposing tubercles to be “*coals*,” or “*charcoal*,” or “*diamonds*,” oxygen would not act upon them. Nobody ever said or supposed it would.

You would want a higher temperature for that?—Or a very prolonged time. Of course when a piece of flesh is buried in the earth, that flesh gradually disappears. The oxygen is absorbed by the earth, and the flesh goes. So would tubercle and the lung itself, but no faster.

It would have the same effect on the lung?—Yes.

Independently of the diseased state of lung would the introduction of oxygen in the way proposed be beneficial?—That is a question out of my province.

Mr. Karlake. I avoided asking that, because this gentleman is only a chemist.

It was not necessary for Mr. Karlake to have informed us he was “*only* a chemist.” I think perhaps it is fortunate for the afflicted that this is so, for were he to attempt to carry out in the treatment of consumption the theories advanced in his evidence, I feel convinced we might confidently predict that the mortality would be increased exactly in proportion to the number of his patients.

I wish to put in Dr. Macgregor’s letters, and Dr. Wills’ letters; but your lordship sees this article comments upon matters appearing in the papers at the time. The papers were the 3rd of November, in which Jones and Meyrick were charged with an assault the 6th of November, when there was an application for a warrant against Dr. Hunter, and the 7th of November, which was the report.

The Lord Chief Justice. I did not understand it to be questioned that there had been those proceedings on the part of Meyrick.

Mr. Karlake then proceeded to address the jury and Mr. Coleridge replied on the whole case; after which the Court adjourned till the following day at 10 o’clock,

NOTE. Why is it that men with small lungs so easily get out of breath? The blood ought to take all the oxygen it requires from the smaller volume, and so keep up their wind. Why is it that, in warm weather we are languid, and indisposed to exertion? I supposed it was because the heat expanded the

air, and diminished its oxidizing powers. Why is it that in hot countries people cannot take much except fruit, if not that there is insufficient oxygen to combine with albuminous food, or to eliminate effete carbon? Why is it that Europeans, who keep up their habit of eating meats in India, all get liver disease, from throwing upon that organ the elimination of the carbonaceous excess?

Dr. Carpenter says—

“If the respiration be lowered in amount by inactivity of body or a high external temperature, a large proportion of *unoxidized*, or *imperfectly oxidized*, excrementitious matters *accumulate in the blood*, giving rise to that augmented production both of the biliary and of the fecal excretions which constitute *diarrhœa*.” —“Physiology,” p. 373.

This is the true explanation of the occurrence of diarrhœa in the majority of cases of consumption.

All these questions suggest themselves to the practical physician, and all the chemists in the universe could not make one such believe his theoretical twaddle. There is a good anecdote told of a miner, which very aptly illustrates the distinction which exists between the writing doctor, the practical physician, and the chemist. “The gentlemen of the pen,” said he, “understand it, but can’t do it; the gentlemen of the apron do it if they don’t understand it; while the *chemists* do not understand it and can’t do it!”

CHARGE OF THE LORD CHIEF JUSTICE.

GENTLEMEN OF THE JURY: I regret very much that I should have felt myself under the necessity of calling upon you to come here again this morning; but I thought, looking to the importance of this case to all parties concerned, that it would be better that you and I should bring our minds fresh to the consideration of the matter with which we have to deal, rather than proceed with the last stage of such a cause after we had been for six hours breathing the atmosphere of a crowded court. Now that we are all fresh in body and mind, and that we have, it is to be hoped, a due supply of oxygen, we shall be better able to grapple with the matter. That it is a very important case there is no doubt. That it is important to the plaintiff is unquestionable, because upon your verdict will very mainly depend his professional position, the success of his future practice, and, what is more, his personal character as a member of society. Because, if a man is an impostor and a scoundrel, in the way that this article suggests, not only his professional but his personal character is irretrievably damaged. The verdict is also important to the defendant; for if he has done only what, under the circumstances, he honestly might do, the plaintiff's character should not be rehabilitated at his expense. It is important, as involving more or less the principles by which the conduct of public writers and their responsibility for what they write is determined. It is important also, because incidentally you may have to consider how far the character and the dignity of a great and honourable profession may be damaged and tarnished by recourse being had to a system of advertising and puffing, such as possibly you may think has been resorted to in this case.

Now there are one or two preliminary matters, of which we may as well dispose by way of clearing the ground, before we proceed to that which is really the great point in contest. In the first place, there can be no doubt that, unless it can be justified on the score of truth, or excused by reason of privilege, this article is unquestionably libellous.

To say of a man who has a professional practice that he is an impostor, that the patients who resort to him are dupes, that he treats them with pretended remedies of which he knows the utter inefficacy, and that he does all this for the sordid purpose of putting money into his pocket, and that therefore so acting he is an impostor and a scoundrel, is unquestionably matter of a very seriously libellous character, if it be untrue. About that there can be no doubt. In the second place, there can also be no doubt that the plaintiff is the person against whom

this article was directed; nor indeed has there been any attempt to disguise that fact on the part of the defendant in this court. Lastly, the defendant, if the plaintiff is entitled to your verdict, is unquestionably liable in this action. He is the printer and publisher of the newspaper in which the article appeared, and as such he is responsible for the article, whether it be written by himself or by others.

Gentlemen, having got rid of these preliminary points, it is when you come to the pleas put upon the record by the defendant that you find the real contest between the parties. The defendant takes two grounds of defence. In the first place he says, What I have written and published is true, and as by the law of England truth is not libellous, I was justified, and I am not responsible in the present action. But he says in the second place (and this is very well worthy of your consideration), Even if I should fail in making out to the necessary extent the plea of justification, or, in other words, the truth of the matter which I have written, still I say that, looking to all the circumstances of the case, the inference of the plaintiff's bad motives and mischievous designs which I have there set forth, were such as any man exercising a due and proper amount of forbearance and moderation in the consideration of questions affecting another man's character, and bringing the best exercise of his judgment to bear upon the matter, might have drawn. Therefore, the matter being one in which the public interest was concerned—for the health of those who might be brought within the sphere of the plaintiff's treatment, would make it of public interest—I was justified in writing upon the subject, and also—exercising a reasonable caution and judgment—in drawing the inferences which I have there set forth, and I am therefore in point of law not liable.

The *first* plea fell to the ground because there was not a tittle of evidence on which to sustain it. The allegations made by the defendant were *false* in substance and in fact, and he was not able to uphold them by any evidence whatever. They were clearly falsehood based upon nothing save the writer's powers of inventive mendacity.

The *second* plea could only be sustained supposing the first to be proved, or rendered probable by proof, as the writer had made *specific* allegations. There could be no exercise of reasonable caution in my saying a man had committed a *theft*, a *murder*, or any other crime, unless I was prepared to show that he had taken what I believed at the time did not belong to him, or had been seen with the body of one I had reason to believe had been murdered by him. If I could not show that he had *taken*

anything whatever, if there had been *no murder* nor any foundation for *supposing* there had been a murder, would it not be absurd for me to talk of "*reasonable caution*," since there could be no exercise of caution in a case where the whole story, from beginning to end, was a pure invention, concocted either as a piece of *sensation scandal* to sell the paper, or as a means of dealing a fatal blow to the moral and professional character of a medical rival?

Now we will deal with those points of defence, in the order in which I have put them. First comes the question whether the defendant has established his plea of justification; or, in other words, whether you are satisfied upon the whole of this case that the facts set forth in this libel are true.

The charge is this: that the plaintiff, dealing with one of the most fatal diseases known to mankind, with the intention of obtaining profit and gain to himself, began by exciting unnecessarily and wantonly the fears of those into whose hands his publication should find its way; that, having excited these fears, he held out to them a delusive prospect of recovery, either from disease which he induced them to think they were suffering under while they were not, or from disease which had actually set itself up in their system; that he, by inducing them to take these remedies, which he knew to be delusive, was trifling with the hopes which he excited, tampering with the health of his dupes, and aggravating their illness; and that he was doing all this, with a perfect knowledge of the inefficiency of his treatment, for the sordid purpose of putting money into his own pocket. Gentlemen, if this charge be true, one can hardly conceive anything worse. The article unquestionably is one of exceeding severity. The language used is of the strongest character. But if the facts upon the assumption of which the article proceeds are true—if it be true that the plaintiff has intentionally, fraudulently, dishonestly put forward these pretences in order to delude patients, and make them in purse, if not in person, his victims—there can be no doubt, I think, that, strong as the language of this article is, it is not one whit too strong; for a man who does all these things is an impostor, and an impostor of the very worst description. To say of him that he is an impostor and a scoundrel, strong as the language may be, is probably what no twelve men in a jury box would think one whit too strong, or inconsistent with the proper use of the English language. But of course it is obvious that the more seriously wicked and aggravated such conduct may be, the more is the charge, if it proves to be unfounded, one of which the person who has been assailed has a right to complain, and with respect to which he has a perfect right to expect at the hands of the jury ample and proportionate compensation.

The reader will be able to estimate the great injustice of such language from the Bench when he understands that at the time it was uttered the defendant had made his defence, and utterly failed to show that in *any one instance* those acts, so *vehemently* denounced by his Lordship, had been committed. His Lordship's strong and unusual language was based, not on my *acts*, not on *the evidence*, but wholly on my *theory* of disease; and because *he* did not believe that, he thought the Jury might think that "*impostor*" and "*scoundrel*" were decent and proper terms to apply to its author. I will venture to say it is the *first* time such a preposterous doctrine has ever been announced from the judicial Bench.

Now, bearing in mind what the charge is, let us see what it is that the plaintiff has published. Before, however, I go to that, it is necessary, in consequence of what took place yesterday and to-day on the part of the learned counsel, to call your attention to one or two parts of this article, as to which it is said that they are portions of the libel; and that, being portions of the libel, the defendant has failed to make out his plea of justification, because he has offered no evidence in support of their truth; and that consequently the defendant is so far without defence, and the plaintiff is therefore entitled to your verdict. Now, that must depend on the construction which ought to be put upon the portions of the article to which I refer. They are these:—
 "There is no need of the revelations of a police-court to show that the "inhaling process means a process for working upon the fears of the "ignorant, and for obtaining enormous fees." That connects itself with a previous sentence, which is this:—"And now a series of recent "proceedings in the Marylebone police-court has revealed the existence of ramifications of the detestable system in question, for which "few ordinary readers will have been prepared." The passage I have just referred to forms a part of the libel as set out in the declaration; and it is said that the meaning of that is connected with the sentence which I read to you just now, and this further one:—"The terrifying "and the plundering are, however, it appears, only one portion of the "fate that may await the ladies who study the advertisements of these "gentry. The charge made against Dr. Hunter by the credulous "Mrs. Merrick sufficiently indicates this, whether he be guilty or not; "and it is known to those who have given themselves the disgusting "duty of inquiring into such matters, that kindred quacks (though of "a more shameless kind perhaps) do unquestionably use in the basest "manner the advantages given them by unfortunate women." Another passage is this:—"Although these advertisements are free "from the mysterious hints and suggestions, and the scarcely veiled

"offensive phraseology of the basest class of medical puffs, one is led to suspect the existence of very serious malpractices, by observing the length and the frequency of these recommendations of Dr. Hunter and his inhalations."

Now it is said that in these two passages the writer of this article intends to convey against Dr. Hunter the charge that he was guilty of the offence with which Mrs. Merrick charged him; and secondly, to assert that he is guilty of a series of malpractices in the exercise of his professional practice. Now, undoubtedly, if those two parts of the article are intended to convey the charge for which the counsel for the plaintiff contends, the justification, so far as those passages are concerned, fails; because no evidence whatever has been given of the guilt of the plaintiff with reference to Mrs. Merrick, *nor has any evidence been given of any malpractice upon his part*. But, taking the article in your hand, you will ask yourselves whether the language is intended to have that effect, or whether it does more than advert to the circumstance which was then perfectly notorious—namely, that this charge had been preferred—with the view of making it the occasion of introducing remarks with regard to that general system of puffery and practice which the article describes.

The main question, however, does not turn upon these two passages: They may enable the plaintiff to get a verdict, but I can hardly suppose that, if those two passages stood alone, this action would have been brought

Therein his Lordship makes a very material mistake. Had but *one* of the passages stood alone, this action *would* have been brought, because at the time it was brought I firmly believed that I could confidently rely on obtaining substantial justice. Now I agree with his Lordship, that I was foolish in entertaining such an idea, and I certainly shall not soon forget that as a *Canadian* physician I cannot apply to the laws of my country for redress without exposing myself to even greater outrages than the libel complained of.

In the first place, with regard to the charge brought by Mrs. Merrick against Dr. Hunter, he has been acquitted; and the article has therefore done him no harm in that respect. It may have been inopportune; I think it was; I think it a great pity that the writer of the article adverted to the circumstance of that charge while it was still pending, assuming it was a charge which had some substance and foundation. But clearly, in that respect, it did the plaintiff, Dr. Hunter, no harm; because he was acquitted.

This is, to say the least, a very *peculiar* view to take of a very

extraordinary occurrence. An infamous charge had been brought against me, which endangered my liberty, degraded my family, and threatened to reduce my children to beggary through the confiscation of my property. *Pending the investigation of that charge*, the *Pall Mall Gazette*, with the spirit which might have animated a fiend, published an article calculated to *incite* prejudice, and to *prevent me from obtaining a fair investigation*. Everything that could have been said was said in that article to *excite public prejudice* against me. It might have compassed my ruin, and no doubt was intended to do so, but fortunately failed to succeed. Was not the *wrong* of the act as great as though it had actually succeeded? And yet for this wrong there is, according to the ruling of the Chief Justice, no redress. If it had succeeded, the defendant would have pleaded as justification the fact of my conviction. As it did not succeed, he now pleads that it could have done me no harm, "*because I was acquitted.*" So that in any event no redress is possible. This ruling of the Lord Chief Justice reminds me of the way our forefathers used to decide the fate of the poor old ladies whom they suspected of being *witches*. They first threw them into a pond, and if they *drowned* they were decreed *not* to be witches, but if they scrambled to the shore they were straightway decreed to be witches, and were *burned* accordingly. So that, whichever way the matter went, they got but scant justice. I cannot think of any other *precedent* by which his Lordship could have been guided.

The main question between the parties is this—Is the system which Dr. Hunter has propounded to the world one which an honest medical writer and medical practitioner would put forward for the mere purpose of enlightening his profession or benefiting the public, or is it a system of quackery and delusion dishonestly put forward for the purpose, no matter at what cost to those who may become victims of the delusion, of getting money and putting it into the pockets of the person thus writing and acting?

I dispute the correctness of his Lordship's observations as to the real question at issue. The "*system*" which I advocate was admitted by the defence to be a scientific and recognised system of medicine. That system consists of the administra-

tion of medicines in the form of vapour. Further than this the Court had no right to inquire. The idea of a non-medical judge and non-medical jury sitting in judgment, and pretending to decide on the truth or falsity of an abstruse medical theory, regarding the essential nature of tubercle? And to decide, too, on *ex parte* evidence, carefully selected, because of its known antagonism to the theory to be tried. Was there ever such a stupendous *farce* enacted in the halls of justice? After the admission, by the scientific witnesses for the defence, that they had practised the same system, and that it was known and recognised in medicine, the functions of the Court were ended, so far as "*the system*" was concerned. After that it could only take cognisance of specific acts of "*dishonesty*," "*imposture*," or "*extortion*," if such existed; but, in the absence of all evidence, to attempt to impute them from a medical theory, of which the Court was utterly incompetent to form a judgment, was, in my opinion, a proceeding without parallel or precedent in the annals of justice!

Now, as I said before, very much must depend upon the subject of the book. Let me see, therefore, in what shape I can best put before you the subject, tenor, and spirit of the work itself. I will endeavour to do the best justice I can to the system propounded by the plaintiff. He comes forward professing to understand, as others have not understood, the true nature of consumption.¹

¹ Every medical man who writes a book on *Consumption*, *Cholera*, or any other disease, professes to understand the disease "as others have not understood it," or he would not write upon it. It is because he *believes* this that he writes. The Chief Justice will presently impute this to *me* as a crime, because several medical men, who have also written books on the same subject, have testified that they like their own theories better than mine.

He comes forward professing to have discovered (*whereas the whole medical profession have abandoned the hope of curing that disease, and have treated it as incurable*) means whereby in its incipient state it may be at once arrested, and in its more advanced phases so far mitigated as to restore the patient to comparative health.

The statement that I had either *said* or *insinuated* that the

profession regarded Consumption as "*incurable*," is entirely at variance with facts. On the contrary, I had devoted one letter entirely to the question of its curability; and quoted no less than *four* of the most distinguished names in the profession to prove it, as the following extracts (pages 102 and 103, Book of Letters) will show:—

The late Dr. SWETT remarks—

"Another important question presents itself. Is Consumption a curable disease? The general impression in the medical profession is, that a patient with Consumption is doomed to death. . . . I have known a number of patients during the last fifteen years who have had the evidences of Consumption, and sometimes in an *advanced stage*, who finally recovered, and are now in the enjoyment of good health" (p. 279).

Sir JAMES CLARK says—

"That pulmonary Consumption admits of a cure is no longer a matter of doubt; it has been clearly demonstrated by the researches of Laennec and other modern pathologists" (p. 137).

Prof. CARSWELL observes—

"Pathological anatomy has perhaps never afforded more conclusive evidence in proof of the curability of a disease than it has in that of *tubercular Consumption*. (Cyc. Prac. Med.) And Laennec declares, that the cure of Consumption where the lungs are not *completely* disorganised, ought not to be looked upon as at all impossible, in reference either to the nature of the disease or of the organ affected. . . . The destruction of a part of the substance of the lungs is by no means necessarily mortal" (p. 328).

Neither did I say or insinuate that the profession had "*abandoned the hope*" of curing Consumption, but the very opposite, that they looked "*hopefully to the future*;" and I quoted (pages 139 to 141) seven of the most eminent authors on the subject: Baron Louis, Dr. Stokes, Dr. Billing, Dr. Cowan, Dr. Watson, Magendie, and Sir James Clark, who tell us that all means hitherto employed have failed, and we must "look for some other method of remedying the evil."

Instead, therefore, of having confidence in the ordinary treatment, all candid authors and teachers look *hopefully* to the future. They admit the necessity for a change, and predict that the day will come when "*the cures of Consumption will be much more frequent*."

But possibly his Lordship was influenced by a belief that the

profession have some uniform and successful plan of treatment, the general results of which are so satisfactory that there exists no necessity for improving it. If this be so, I am sorry to be compelled to disabuse his mind of the comforting delusion, by making a few quotations from the works of leading members of the profession.

From EDWARD SMITH, M.D., LL.B., F.R.S., Physician to the Hospital for Consumption, Brompton (published in 1864):—

“Hence, as a final expression, we may state that we are not agreed as to the essential nature of the disease, have no unfailing mode of treatment, and the disease is *essentially* and almost as *universally* a fatal one as it has been in all “ages” (p. 30).

From ALFRED BEAUMONT MADDOCK, M.D. (author of a work on Diseases of the Chest):—

“This strange apathy has been exhibited, it must be remembered, in the treatment of those diseases declared as incurable under the old routine practice” (p. 4).

By way of introducing his treatment and showing that it must be efficacious, he enters into a discussion of what are the causes of the complaint. He says, It has been the fashion to believe that consumption, or, in other words, tubercle on the lungs, is the consequence of hereditary predisposition; that is a fallacy; the true and only cause of it is imperfect respiration; and imperfect respiration acts in this way: that whereas the healthy condition of the lung depends on the free inspiration of atmospheric air whereby the blood may be purified, when you get the respiratory organs obstructed, the consequence is that the due amount—that is, the amount which nature requires in order to keep the system healthy—of oxygen which should be derived from the atmosphere fails. The consequence of that is, that you get an excess of carbonaceous matter in the blood; that that carbonaceous matter, being brought to the lungs in the course of circulation, accumulates in the lungs, it not being got rid of as it would be if the necessary amount of oxygen were inspired in order to convert it into the gaseous forms in which it passes off from the human system. Carbon accumulates in the lungs. Carbon is tubercle, or carbon is the cause of tubercle, and tubercle is substantially carbon. How then are you to remedy the disease which is thus set up from an obstruction of the respiratory organs or air-tubes which convey the atmospheric air to the lungs? You cannot by breathing get the necessary amount of oxygen into them. The only mode is to have recourse to artificial means. I have invented, or I possess, an instrument whereby oxygen can be inhaled or artificially conveyed into the lungs. *The medical profession know nothing of this; they treat the disease by*

remedies administered through the stomach, but this is all idle and delusive.

I have nowhere used such a form of expression as "*the medical profession know nothing of this.*" What I have said is, that they do not regard the disease as I do, and do not treat it as I do. That I have more faith in my view of the matter than I have in theirs. I leave the public to choose between these different doctrines according to their judgment. Would his Lordship wish to force the afflicted to follow the ideas of any particular medical school? If so, I venture to say that few will be willing to forego the exercise of their own judgment and discretion.

Remedies administered through the stomach cannot reach the part locally affected; but if oxygen is inhaled by means of my inhaling process, you get not merely regeneration of the blood by the admixture of the oxygen which nature requires, but you get a direct and immediate application of the remedial agent to the part afflicted. The oxygen which by my artificial means of inhalation is carried into the system, operates with a twofold effect. In the first place it acts immediately on the carbonaceous matter of the tubercle and decomposes it. Its removal takes place and the lung heals. In the second place, the oxygen, which is by my system artificially inhaled, passes into the blood and purifies the blood, and enables it to restore healthy matter in place of the worn-out tissue of the body, and so produces vitality and health.

Now, gentlemen, if that be true, there could not be a greater blessing to mankind than the discovery of which the plaintiff boasts himself to be the author. But we are told, and upon high scientific authority, that the whole of this is purely delusive.

That, if true, the theory and practice advocated by me would be productive of incalculable blessings to humanity, none can doubt. Realising this fact so fully as his Lordship did, it is amazing that he should not also have realised the absurdity of a non-medical court attempting to decide upon its truth from the misrepresentations of its enemies, directly interested, too, in destroying its reputation!

In the first place, we are told that it is not true that imperfect respiration is the cause of tubercle in the lungs; and that this notion, the assumption of which appears to be the foundation of the plaintiff's theory and system, is wholly fallacious—

that he is deluding people when he says that imperfect respiration alone will cause consumption, that there must be a predisposition to it, either from hereditary congenital taint or from some circumstance or other which has set up this scrofulous disposition, or habit of body. They say furthermore that he is completely deluding himself or deluding the public when he says, that tubercle is neither more nor less than carbon, or, at all events, carbon united with the disintegrated tissues of the body. They say that that is an entire fallacy, and that as these two assumptions form the foundation of the whole of the plaintiff's system, both as regards the nature of the disease and the manner in which he proposes to treat it, his system is an entirely delusive one.

We have here a distinct denial of the fundamental doctrine on which my theory of the essential nature of Consumption is based. I marked particularly the words uttered by his Lordship at the time he was delivering his charge, and I confess I did so with feelings of indignation. Here was a great man labouring with all the force of his intellect to destroy the reputation of one who, whatever may be his faults and weaknesses, is incapable of stooping to make a statement which he does not believe to be true, and for which he cannot give a good reason.

I here repeat, in the teeth of the so-called scientific evidence on which his Lordship placed such implicit confidence, that in the adult imperfect respiration generally, if not invariably, precedes the deposition of tubercles in the lungs. I believe it to be invariable—a law as immutable as that governing the revolution of the heavenly bodies. But let us hear what the best authorities say on the subject.

Under the head of “Signs and Symptoms of Tuberculosis”—that is to say, the condition which precedes the deposition of tubercles in the lungs—we have the following from the great work of Ancell (late Professor of Materia Medica and Medical Jurisprudence to the Medical School of St. George's Hospital; author of *A Course of Lectures on the Physiology and Pathology of the Blood*, and of *Commentaries on the Doctrines of Liebig*), whose treatise on Tuberculosis, from which I wrote, is the most erudite and exhaustive monograph ever published in the English language.

"The breathing is confined, frequently attended with a somewhat painful constriction of the chest, or the respirations are *short* and increased in number, liable to aggravation in the evening, without any further affection of the lungs than appertains to the general disease . . . the habitual or ordinary respiration corresponds with the excitability of the pulse, in being easily hurried into shortness of breath, under the influence of comparatively trifling mental or physical exertion. (p. 89). It shows a *diminution of the vital capacity or breathing power of the lungs, which occurs in tuberculosis* [consumption] *BEFORE any deposit or appreciable organic disease.* The feebleness of respiration indicative of diminished vital capacity, the practical observer will frequently detect with the naked eye." (p. 90).

Again—

"I believe that in a tuberculous condition of the blood, the natural slight mobility of the upper part [of the chest] is for the most part *materially diminished*, and that the loss of mobility in the chest is in direct relation to a *diminution of breathing capacity.* (p. 93). I admit a diminished vital capacity of the lungs as one of the diagnostic signs of tuberculosis. (p. 94). We ought to be apprised of the amount of carbonic acid exhaled and oxygen consumed in tuberculosis. But few experiments have been made to determine this . . . Out of six consumptive patients, *five* expired less carbonic acid than healthy individuals of the same age and sex. (p. 97). THE RECIPROCAL ACTION BETWEEN THE CHYLE AND THE AIR, FOR THE CONVERSION OF THE FORMER INTO BLOOD, IF IT BE ADMITTED THAT THIS TAKES PLACE IN THE LUNGS, MUST BE THE PART OF THE FUNCTION OF THESE ORGANS WHICH IS DEFECTIVE. Hence healthy liquor sanguinis [the fluid of the blood] is not elaborated, the consequence being defective nutrition, both of the blood and the solid structures of the system." (p. 98).

EDWARD SMITH, M.D., LL.B., F.R.S. (one of the physicians to the Brompton Hospital) says—

"The vital capacity of the lungs is *diminished even where there are no evidences whatever of the presence of tubercular deposits.* We have tested this question with every care and in many persons, and believe the proposition to be true in every period of the early stage of phthisis. No one will deny that the vital capacity is lessened where there is a material impediment to inspiration, such as occurs with deposited tubercle, but may question it in the absence of this deposition, and affirm that *if it exists there must also be tubercle* . . . The amount of diminution varies with the duration of the disease." (p. 107).

Again—

"The *earliest* and therefore the *most universal* condition is that of lessened force and fulness of the respiratory murmur and *diminished length* of the ordinary inspiratory act."

Dr. STOKES remarks in reference to *feebleness of respiration*—

"Of the different signs of incipient consumption there is none more important than this," and [that it] "may occur as the sole phenomenon." (p. 113).

And further—

“We believe it to be a *universal condition in the earliest stage of consumption*, “that the vesicular murmur [movement of air in the air cells] is less strong “than occurs in health.” (p. 174).

Lastly—

“The earliest evidence of the deposition of tubercle is that which indicates a “localised and isolated obstruction to the current of the air.” (p. 176).

Dr. HENRY MACCORMAC says, in his preface—

“Consumption and scrofula in every essential are one. Tubercle, in its varied “protean guises, is but the result of the blood’s deterioration, of the retention “of carbonaceous, hydrogenous and other impurities where they have no “business to remain. In consequence of the *imperfect performance of the “respiratory act*, these impurities accumulate in the tainted blood.”

In regarding imperfect respiration as almost invariably the cause of that condition of the system described by Sir J. Clark as the tuberculous cachexia, and which ends in the deposit of tubercles in the lungs, I was guided and sustained by authority, observation, and experience. Into its truth or fallacy as a medical doctrine the Court had no right to inquire. It was not competent to decide. There is no theory in medicine against which it would not be easy to array half-a-dozen physicians who entertained different opinions. Probably no two of the medical witnesses for the defence held precisely the same theory. For aught we know they may have been, on other points, more opposed to one another than they even professed to be opposed to me on this. But what I wish distinctly to say is, that they ought to have known, and must be badly read if they did not know, that many of the most distinguished names in medicine were committed to substantially the same doctrine.

These are some of the facts upon which I base my theory. It does not matter whether the medical authors, from whose works I have quoted, interpret those facts in the same way that I do or not. They admit that there is, generally or invariably, imperfect respiration before tubercles are formed, though they may not be prepared to say that imperfect respiration is the sole cause which produces them. I claim the same right as any other member of the profession possesses to draw my own inferences from those facts. It is enough that, at the time I wrote my

book, I was conversant with them, and that my deductions were based upon them.

So long ago as 1852, I propounded similar views in a pamphlet published in New York, from which the following extracts are taken :—

“We have now glanced at the more prominent theories which have been advanced to solve this difficult problem. Which one of them may be regarded as *orthodox*, or whether any of them be *true*, we must leave to the reader to determine, and pass at once to the development of our own views on the subject. We believe that there is but one *TRUE CAUSE* of tubercles in the adult, and that cause is imperfect decarbonisation of the blood, through the failure of the function of the lungs. This *always* precedes the development of tubercles, and increases with the progress of the tuberculous disease. The primary effect is produced on the blood, but the subsequent injury resulting from the circulation of that blood, may involve any organ or tissue of the body. The derangements of the stomach, liver, nervous system, circulation, &c., are mere consequences of mal-respiration, or accidental complications of the disease.

“The chain of *causes* runs through all those influences—*defective formation, obstruction, and mode of life*, which limit the *quantity* or degrade the *quality* of the air supplied to the lungs. A narrow, deformed chest may be *inherited*; and such a chest, as a rule, strongly predisposes to tubercles. A whole family may be so predisposed, and one after another fall a sacrifice to consumption. If so, they are most likely to do so about the age of puberty.

“The *second* link is found in *obstruction of the air passages*, resulting from inherited irritability of the lining mucous membrane, or from acquired disease. Strumous catarrh, granular sore-throat, chronic laryngitis, and bronchitis, are affections which directly tend to obstruct respiration and lay the foundation of tubercles, through defective oxydation of the blood.

“The *third* and last of these links will be found in those modes of life which render the respiratory function imperfect, through exposure to irritating particles of dust floating in the air, or a cramped position of the body and insufficient expansion of the lungs, or impure air from a close and confined atmosphere.

“That imperfect respiration is the real cause of consumption when it occurs in the adult, is further proved by the fact that it occurs in *brutes* whenever placed under those influences which impede respiration. Carswell admits that tubercles are readily produced in the rabbit by confining it in impure air, and by giving it bad food. Cows all die of consumption when kept shut up in stalls during the summer months, no matter how well they may be fed. Monkeys, when caged up, all die tuberculous, though indulged with abundance of food of the best quality. The experience of the medical officers of prisons tends to show that *air* has everything, and *diet* very little, to do with the production of tubercles in the lungs of criminals.

“Now, if tubercles are produced in the brute creation, under the operation of

“certain influences, with the regularity of cause and effect, such influences must be held to be the *true* causes. If one of these influences may *not* be dispensed with and all the others may, then we narrow the matter down to one cause, and that cause I believe to be *imperfect respiration*.

“Of course want of exercise increases the injury, for exercise is one of the most powerful means of increasing the respiratory function. The difference in the quantity of oxygen consumed and carbon expelled is surprising, even in health, when sitting quietly on the one hand, or walking four miles an hour on the other. This, therefore, only tends to confirm the opinion that defective respiration is the cause; for it is known that persons taking active exercise on foot in the open air are very little prone to consumption.

“Laennec tells us that he had under his professional care a religious association of women, the rules of which were of such extreme severity, that none save the superior, the gatekeeper, and the sisters who had charge of the kitchen, infirmary, and the garden, were permitted to go into the open air. In the course of two or three months they, one after another, became consumptive; and in the course of ten years, he witnessed its entire renovation two or three times, through the death of all its members. Only those escaped who discharged the *out-door* duties, and such as he prevailed upon to leave the association. The latter quickly recovered in every instance, although some of them exhibited well-marked indications of the disease.

“That defective respiration was the chief if not the sole cause of the disease in the case of this religious association, cannot, I think, be doubted, when we reflect that the same religious rules and the same dietary extended to all.”

“The philosophy of respiration is as follows: the impure blood, chyle and lymph, mixed together, are brought to the lungs and distributed over millions of little cells, in minute vessels, which form a network on the internal lining of each cell. When we breathe, these cells are inflated, and the air containing oxygen is brought into relation with the blood. The delicate membrane which separates them permits the oxygen to pass in and the carbonaceous impurity to pass out. Whether the change takes place immediately in the blood vessels of the lungs or in the systemic capillaries is of little consequence. It is a chemical union, and can only take place in definite proportions—that is to say, a certain quantity of oxygen is necessary to displace a certain quantity of carbon. It has been estimated that, in healthy respiration, we get rid of from *eleven to thirteen ounces* of carbon from the blood, and receive from *thirty to thirty-seven ounces* of oxygen from the air daily. So long as this *balance* is maintained, the blood will be properly *purified*, the chyle properly *vitalised*, nutrition properly maintained, and neither scrofula nor tubercles can find a habitation in the body.

“The air will only give up a fixed proportion of its oxygen; hence it is that the volume of air received measures the quantity of oxygen which can be given to the blood and the quantity of carbon which can be expelled. I believe the volume of air regulates to the weight of a grain the nutriment which can be built up into the system; and the quantity of carbon, which, as carbonic acid, can be expelled from the lungs. The loss of balance between these two elements deranges the whole machinery of life, and throws upon the skin and liver a part of the function of the lungs. The carbon, as fat,

"not being wholly oxydized, is partially expelled as a hydro-carbon. *Animal heat* depends on this proper oxydation, and hence, when it is imperfectly performed, "the *extremities become cold* and the *circulation feeble*. The perspiration becomes "oily and of an offensive odour. The lacteals refuse the product of the liver, "through inability of the lungs to oxydize it; and hence it either runs off by the "bowels, constituting the *diarrhœa* of consumptive patients, or gives rise to fatty "degeneration of that organ. As the quantity of chyle vitalised in the lungs "is below the standard of health, less and less nourishment is prepared, while "the waste goes on as in health. This loss of balance between the supply and "waste is seen in the rapid emaciation, which has long been regarded as the "very type of the disease, Consumption—a *consuming* away.

"These two *influences*—defective nutrition, resulting from the *diminished quantity of chyle vitalised* by the lungs and the vitiation of the blood by the presence of "retained, unoxidised or imperfectly oxidised carbon—*nnite* to produce that "peculiar condition of the system known as the *scrofulous* or *tuberculous* "cachexia."

For fifteen years this doctrine has been advocated by me, and whether it be true or erroneous, it cannot be overturned by such articles as those of the *Pall Mall Gazette*, or the quibbling and false interpretation put upon it by scientific witnesses in a non-medical court. I believe it to be fraught with *good* to the afflicted and *honour* to myself. I am sorry, of course, that it does not meet with the approval of Williams & Co., or the Chief Justice of England, but the former have not put forth any theory half so rational, while the latter is, in my opinion, not a competent judge of the matter.

With regard to defective respiration being the cause of tubercle, they give facts which would appear to afford an obvious and an immediate answer. In the first place, they say there are many instances in which respiration is imperfect in a subject for years, and yet no tubercle is formed; that respiration is imperfect in cases of asthma, from which people sometimes suffer for a long series of years; also in cases of *disease of the heart*; also in cases of *distorted spine*: and that, though in some of these instances the respiration is imperfect in a very great degree, yet tubercle does not form.¹ So again they say, taking the converse of that proposition, that there are many instances indeed where persons die of tubercular consumption, and yet the organs, apart from the lungs, are found in a perfect healthy state, the air-passages not being at all obstructed.² Again they say, which is perhaps a more striking illustration than any of the rest, that there are many instances in which persons die of tubercular disease of the lungs, and after death it is found that other vital and important organs of the body are in like manner affected by tubercle, but that these are organs which can never be affected by the act of respiration at all.

Therefore they say that this view propounded by the plaintiff is utterly untenable.³

¹ However conclusive these assertions may have appeared to his Lordship's mind, they do not, in my judgment, furnish any answer whatever. If the reader will turn to Dr. Williams's evidence (p. 110), he will find that I have fully exploded the fallacy of his *three* instances, "*Asthma*," "*Heart Disease*," and "*Distortion of the Spine*." But, to demonstrate the utter absurdity of his evidence, ask yourself what it would prove if it were true. Why, simply that imperfect respiration may, in rare instances, exist without ending in Consumption. This, surely, is no reason why imperfect respiration may not be the efficient cause of Consumption. Would it not be an absurdity to say that, because of three nurses in a fever hospital one took the disease and two escaped, or two took the disease and one escaped, therefore the disease could not have been conveyed by the air, as they all breathed the same air and were exposed to the same influence, and yet were not all affected? The assumed ignorance of the medical witnesses of the law of vital resistance to disease was one of the most humiliating exhibitions of preconcerted disingenuousness ever presented in a court of justice. It would be a sad thing for mankind if all who are exposed to infectious poisons were to fall a sacrifice to them. Nature has endowed the body not only with *vital* resistance, but the several organs with *vicarious* powers for mutual assistance. Hence it is that the *skin* and the *liver* so frequently come to the relief of the lungs, and save the health by removing from the blood impurities which the lungs are incapable of expelling. If of one hundred persons exposed to any injurious influence *ten* contract a particular disease, and ninety escape, the fact that that influence was the cause of such disease is as clearly proved as it would be if ninety had contracted the disease and only ten escaped. There is a great difference in our *resistance* of disease, and that difference depends on our vital powers, and the difference in the co-operating harmony of our organism.

² When his Lordship says "There are many instances indeed "where persons die of tubercular consumption, and yet the

“organs apart from the lungs are found in a perfectly healthy state, the air passages not being at all obstructed,” *his* meaning is not so *obvious*, to my mind, as I could wish. The air-passages are *always* obstructed in tubercular consumption of the lungs, and the *air-cells* are, in the opinion of the most eminent pathologist living, Professor Carswell, the common seat of tubercle, becoming, as the disease progresses, filled up by this matter. Now, if “the organs apart from the lungs are found in a perfectly healthy state,” how could there be Consumption? If the lungs are free from obstruction, and every other part in “a perfectly healthy state,” there is nothing the matter. If that be his idea of Consumption, it is a very absurd idea.

Even in tubercular infiltration of the cellular tissue, the law of shallow and imperfect respiration precedes the infiltration; and the infiltration, as it takes place, shuts up, not only the air-cells, but also the capillary bronchial tubes. Then to assert that a patient can have Pulmonary Consumption without *bronchitis* supervening, and without *bronchial obstruction* from the presence of *mucus*, is at least to assert what cannot be proved, and what practically does not exist. The sentence is only another instance of the palpable absurdities which even great men may utter, when pronouncing opinions on subjects which they do not understand.

³ It is impossible to read without a smile that “*other vital and important organs of the body are in like manner affected by tubercle, but that these are organs which can never be affected by the act of respiration at all.*” If they are *vital* organs, they are certainly *important* to every man who wishes to live; and I will venture to say that those who do ought to impair their respiration will discover that the health of such organs has not at least been *improved* by the experiment! Physiologists tell us that the purification of the blood mainly depends on the “act of respiration.” If respiration be suspended for a few minutes we die. If it be *imperfectly* performed, we are told by Professor Carpenter (*Physiology*, p. 304) that “those oxydating processes which minister to the elimination of effete matter from the system must be imperfectly performed, and that an accumula-

“tion of substances tending to putrescence must take place in “the blood.”

Now if his Lordship thinks that the circulation of this impure blood through “other vital and important organs” will not affect them injuriously, or that the impurity does not depend on the imperfection of the act of respiration, then all I can say is that he is a law unto himself, for his views are diametrically opposite to those of every living medical authority, not even excepting the scientific witnesses for the defence !

Then comes the question whether or not the tubercle is, as he describes it, principally composed of carbon. They say, That is certainly untrue, and the authority upon which (they tell the plaintiff) you base that assertion, namely, the analysis of Scherer, you have either misunderstood, or more probably misrepresented.¹ Tubercle contains no more carbon than any of the other animal tissues of the body, still less does it contain carbon in the form you have described ;² it only contains carbon like all other parts of animal matter, that is, in chemical combination, and it is not until decomposition takes place, that the carbon can be separated and eliminated from the other elements with which it is in combination.³ To suppose, therefore, that the introduction of oxygen could act upon the carbon of the tubercle specifically without acting upon the other parts of the lung is absurd. Just as it would act upon the carbon which is in chemical combination in the tubercle, so would it act on the carbon contained in chemical combination in the other parts of the lung. Therefore the whole of this theory is perfectly delusive, and if you had read Scherer with the intelligence of an enlightened medical man, you would have known perfectly well, and you must have known, that you were using his analyses for a purpose for which they could not by any possibility have been intended. Those are the two strong points in which the scientific evidence of the defendant grapples with the theory of the plaintiff.⁴

¹ His Lordship here makes four assumptions, and he bases them all on the medical evidence. 1st. He assumes that there is no excess of carbon in tubercle over other animal tissues. This assumption is not correct. Scherer, in his analysis, found the following to be the elementary composition of Tubercle :—

Carbon	. . .	53·888
Hydrogen	. . .	7·112
Nitrogen	. .	17·234
Oxygen	. .	21·766
		<hr/> 100·000

And this, too, after he had separated all the *fat*, a very essential ingredient in its composition, as containing a much larger percentage of carbon.

Dr. Glover found the following proportions in an elementary analysis of the three different specimens of tubercular matter:—

	1st.		2nd.		3rd.
Carbon .	54·97	...	56·40	...	53·43

Muller gives us the following elementary analyses “of other animal tissues,” on the authority of

Michaelis :—

Arterial blood contains of Carbon . .	51·382
Fibrin	51·374

Gay-Lussac and Thenard :—

Albumen (coagulated)	52·883
--------------------------------	--------

Gass and Pfaff :—

Muscle	48·30
------------------	-------

It contains even more carbon than impure venous blood, the quantity in which is, according to Michaelis, quoted by Muller (vol. i., p. 122), 53·231.

So that, notwithstanding the assertion that tubercle contained less carbon than the healthy tissues, as that “very intelligent gentleman” who compared tubercle to “a beefsteak” informed his Lordship, it contains considerably more than even the *poisonous venous blood*!

But these analyses refer to crude tubercle, mixed up with a variety of foreign elements which materially modify the proportion of carbon in analysis. When I spoke of Scherer’s analysis, I did so with reference to the known small proportion of hydrocarbon or fat in the parenchyma of the healthy lungs—not more than 6 parts in 100—whereas in tuberculous lungs the proportion rises to 40, and even as high as 52 per cent. Ancell says (p. 153) :—

“A most curious fact was discovered by M. Guillot, which, if corroborated, might ultimately assist us in arriving at a knowledge of the *essential nature* of tuberculosis . . . that in all diseases wherein a portion of the lung loses its *functional powers*, an accumulation of fat takes place in that portion.

"Two explanations have been suggested of this increase of fatty matter in tuberculous or non-respirable lung. It has been held that the materials of respiration being, to a certain extent, converted into fatty matter in the blood an accumulation of fat takes place, *in consequence of the lung having ceased to perform its function*. It has, on the other hand, been held that tuberculous matter is *essentially composed* of a large proportion of fat. According to this latter view, the nature of tuberculosis is diametrically opposite to that suggested by some authors—it is fatty degeneration."

Here, then, we have a hydro-carbonaceous product laid down in the tissues of the lung, containing from 54 to 56 per cent. of carbon, whereas the healthy parenchyma should only contain 6 per cent. of fat, or about 4 per cent. of carbon. But this is not all; the fatty principle is chiefly cholesterine, and when separated yields a much higher percentage of carbon. This cholesterine is always a product of excretion, and exists in cancer and other malignant diseases. "The deposit of Atheroma," says Professor SWETT,

"Has long puzzled pathologists as to its true nature and seat. Every one has noticed it who is at all conversant with examinations of the body after death Recent and improved examination has discovered that this deposit contains abundant globules of fat, with crystals of cholesterine and some earthy matters." (p. 468.)

"Messieurs Becquerel and Rodier have observed an increase of fat, and especially of cholesterine, in chronic diseases of the liver, in Bright's disease of the kidneys, and in *tuberculosis*." (Carpenter.)

This *carbonaceous* product constitutes, in my opinion, the essential element—the *germ* or leaven which transforms albumen and other animal products to which it has been supposed to be analogous, into crude cheesy tubercle, and subsequently induces fatty degeneration.

2nd. His Lordship says, "But it is not carbon in the form described." Now, it so happens that I did not describe any form! I meant, of course, carbon *as one of the elements of the body*—not "diamonds," nor yet "*charcoal*," nor yet "*plumbago*." The medical witnesses knew perfectly well what was meant; but it suited their purpose to appear not to do so, and so they fixed on *dry inorganic carbon*! All their evidence was based on this disreputable quibble.

But even in this they were not half correct, since we do

find in the lungs of persons who have died of tubercular Consumption, and in parts of the lung *from which the air has become excluded*, a carbonaceous product almost pure and often nearly dry.

BAYLE describes melanosis of the lung

"As consisting of ulcers of various sizes, as black as coal, and very hard"
 "* * * * If the whole lung is affected, it becomes hard, compact, black as
 "charcoal, and sometimes like half-burnt leather; he made this a *distinct species*
 "of Consumption." (Ancell, p. 156.)

"Chemical and microscopical research have at present by no means clearly
 "elucidated different forms and varieties of melanotic matter. According to the
 "analysis of Breschet and others, it contains a large proportion of CARBON, and
 "it may be regarded as the colouring matter of the blood, fibrin and albumen in
 "a modified state, and phosphate of lime and other salts of the blood with
 "three distinct fatty matters. * * * This is the true melanosis of Vogel, who
 "states that these molecules are not pure carbon, but contain a large proportion of
 "that element." (Ancell, p. 157.)

Again :—

"The tendency of melanotic deposit depends primarily upon the condition of
 "the blood, and, secondarily, upon local circumstances; hence, in *pulmonary*
 "*consumption*, it appears to be intimately connected with the disturbance of the respira-
 "tory function."

So much for his Lordship's assumption on that point. I now come to the—

3rd assumption—viz., that carbon cannot be *eliminated* "until decomposition takes place!" What does he mean by "*decomposition*?" Does he mean death? Decomposition is taking place every instant of our being. He does not *speak*, *think*, or *move* without decomposing a part of the organic structures of which his body is composed. Carbon is being eliminated every moment of life, and it might be a sad thing for England if, in his Lordship's case, the operation were to be suspended for only ten minutes! And what is more, if he had not a due proportion of oxygen to eliminate it! I am afraid he is not profoundly versed in the mysteries of medicine, and I would strongly advise him to study that little book the plain precepts and scientific truths of which he took such pains in holding up for fools to carp at.

4th. He thinks that oxygen would combine with the living

tissue of the lung, which would be a very sad disaster; so of course oxygen *must* be dangerous. The only thing which appears to me "*absurd*" is *his* supposition that oxygen *would* combine with a highly-organised living tissue *directly* or in any manner than through the blood.

There is nothing in my book which was designed or calculated to create such an impression, or which warrants such a deduction. The most direct way of reaching the tubercular deposits is *through the blood*, and the most direct and efficient means of acting on the blood is through the lungs. That is all I have said, or intended to say.

But the witnesses go further. They say, Even assuming that you were right, that the principal element of tubercle was carbon, assuming even that the introduction of oxygen into the lungs would, either by purifying the blood or by decomposing the carbonaceous deposit, restore the lungs to health; when you come to the application of your treatment, it will be found to be untenable in theory and utterly inefficacious in practice.¹ In the first place, when you talk of introducing oxygen into the system, it must be borne in mind that you can only do so to a limited extent, because the act of inhalation is one which more or less excites the patient, and which can only be carried on for a limited period.² If you consider the quantity of air, and therefore of oxygen, inhaled at every breath that you draw in, the quantity is so large, it is so much in excess of what nature naturally requires, that, even assuming the respiratory organs to be in some degree obstructed, there would be still always a great excess over that which is absolutely required.³ Again, if you look to the quantity which you are constantly drawing in by means of the breath, and if you look to the quantity which in the course of the short period over which the patient could inhale, you could possibly convey into his lungs by any artificial means, the quantity so conveyed will be found to be so infinitesimally small in proportion to that which is being constantly drawn into his lungs by the simple operation of breathing, that practically it can be of no effect and of no avail.⁴

I think, according to the figures given me yesterday by that very intelligent gentleman who dealt with the chemistry of this matter—I mean the last witness who was called—that the amount which could be taken in by inhalation would not amount to one per cent. of what in the same period of time a man might draw in by his breath.⁵

¹ I cannot pretend to be profoundly versed in logic—no man educated in the colonies dare do that. Hence it is that, failing

to perceive, I modestly inquire *how*, if it be admitted that the theory of the carbonaceous nature of tubercle is sound, and the oxygen treatment scientific, it *can* be “untenable in theory?” The treatment might be difficult of application, or so imperfectly carried out as to prove “*inefficacious in practice*,” but why assume what was not in evidence? The defendant did not produce a single witness to show that it had been inefficacious, while many proved the opposite—that it was *very efficacious*.

² No physician who had ever administered oxygen or respired it, would bear out the assumption that it “excites the patient.” I stated in my evidence that I had inhaled 400 cubic feet of pure oxygen gas at a sitting without the least excitement. No witness was produced who had made such an experiment. On one side there was *theory* based upon *assumption*, on the other *fact* based upon *experience*. His Lordship preferred the theory—the patients preferred the facts and experience.

³ If we do get more oxygen “than is required” from the smaller volume of air which consumptive people respire, then *small* lungs ought to be better than *large* ones, and a contracted chest should be a proof of pure blood and a vigorous constitution. It is absurd for consumptive people to crave more air, and complain of *shortness of breath*, when the Chief Justice of England, on the authority of “the most intelligent professor of chemistry which it had been his good fortune to meet with,” tells them that they have already got more oxygen than they require. Nature is too importunate in her demands, and modern *science* comes forward to show how very unreasonable she is!

⁴ When a consumptive patient inhales for fifteen minutes three times a day an oxidising inhalant, the quantity he gets is not so small as his Lordship supposes. Every drop of blood in the human body makes the circuit of the system many times during fifteen minutes, and at each circuit it receives additional oxygen. During the fifteen minutes, every drop of blood in the living body has been brought several successive times under the purifying and vitalising influence of the oxygen. Then, in addition to this, the patient regularly exercises the lungs, inflating the air vesicles, forcing open those which are partially

closed, and thereby increasing the vital capacity of the chest. This gives an impetus to the system, rouses its dormant energies, stimulates the absorbents, and favours the expulsion of the morbid products forming in the lungs.

⁵ The "*Intelligent Chemist*," with his calculations, has already been answered under the medical evidence.

But then the witnesses proceed to say, The inhalation of oxygen gas has been tried, and it has not been found to succeed.¹ You yourself have tried it. You say now that you do not compel your patients to inhale oxygen gas as a gas. You give it to them in combination with other matters, which combination is first to be dissolved before the gas can be evolved so as to pass into the system. Therefore, says the chemist, you must in the first place, if you purpose to use the oxygen in combination with some other matter, separate it from that matter with which it is so combined. You purpose to use chloric acid, you say that that is what you use; let us see what is the process to which you would resort, and what would be the effect of it. You either use it alone or you use it with other substances. If you use chloric acid alone you must use it in the full strength of the ordinary solution, or you must reduce that strength. If you use it in the full strength of the solution as an acid the consequence would be this: you must heat it to a high temperature before the chlorine gas and the oxygen will separate.² When you do that, although you obtain the oxygen you require, you can only do it with the accompaniment of the evolution of the chlorine gas.

Now chlorine gas is such an irritant that if it were allowed to pass, as it would be allowed to do in the act of inhalation with the oxygen, into the lungs, it would set up immediate irritation in the lungs to such a degree as would render it perfectly impossible for the patient to continue the inhalation. Supposing you use it in a weaker solution, then the effect is that although you would have a less quantity of the noxious vapour—chlorine gas—which would irritate and be so detrimental to the lungs, you would of course reduce the quantity of oxygen, and you would reduce it to so infinitesimally small a quantity that it could not by any possibility have any beneficial result.

Again, supposing instead of using chlorine gas alone, as it appears Dr. Hunter recommends, you use it in combination with other things—sedatives, like stramonium, conium, &c., or alteratives, or whatever other combination he chooses; then, says the chemist, the moment all these matters are brought into combination and subjected to the action of heat, which they must be, the effect might be to decompose the chloric acid as well as the other matters. But then the oxygen would be immediately seized upon by the other things present, and it never could pass into the lungs in the state of oxygen, so as to effect the purpose for which the plaintiff affects to administer it; more especially

if you use tinctures, as appears to be the case, because the alcohol in the tinctures would seize upon the oxygen, for which it has the closest affinity, and carry it away. They say, therefore, not only is the theory of the system altogether based in delusion, but its practical application would be impossible, even if there were not error at the root of it.³

¹ Had his Lordship winnowed the grain from the chaff of the medical evidence, he would have discovered that there was nothing whatever to justify his assumption that oxygen had been tried, and was not found to answer. Only Dr. Williams and Dr. Cotton pretended to have ever *witnessed* its administration, and the latter only *during the past six months!* If it had been tried, and was not found efficacious, *why* were not those who had so tried it put into the witness-box to *prove* its inutility? If it was found wanting years ago, how does it happen that Dr. Biegel recommends it in strong language in a book *just published*, and Dr. Abbott Smith likewise? How does it happen that Dr. Cotton *countenanced* and *aided* in the administration of oxygen *within the past six months*—two years after the publication of my views in England, and fifteen years subsequent to their publication in America? If he knew Dr. Williams had tried it, and found it of no value years ago, he must either have had no confidence in his *assertions*, or, while believing it to be worthless, have been practising an imposture upon the public, and upon those *three* unfortunate patients to whom he referred. But the truth is, he knew nothing about it from experience; had never used it in Consumption in any case whatever; and even Dr. Markham, when pressed in his cross-examination, said, “*I do not like to say that if oxygen got into the lungs it would not do good.*”

² All his Lordship’s laboured argument was directly in the face of the fact that I stated in my evidence, that I used chloric and other acids as *oxidising agents*, and not to evolve oxygen gas; that the acid vapour was more efficacious than the oxygen, and chlorine would be separated. This he certainly recollected, for in this very paragraph he adverted to the fact that I had said I did not administer the oxygen as a gas, and yet he seemed

resolved to impress upon the jury that I did not do what I was not trying to do.

³ None of the medical witnesses pretended to have ever used chloric acid, or to know more about it than that it was a compound containing *one* equivalent of chlorine and *five* equivalents of oxygen. They *thought* the chlorine must "produce irritation." How, if chlorine be an "*irritant*" and a "*noxious vapour*," as his Lordship assured the jury, does it happen that Gannal and Cottureau recommended it strongly as a remedy for consumption, and that Louis should have said in regard to it, "The cases [of "*consumption*"] related by Cottureau are by far the most satisfactory; we would refer to the first, also published by Gannal, "and to the twelfth, as particularly striking and decisive as to "the existence of *tubercles*?" Or that Dr. Thompson, in his *Materia Medica*, should have said that the inhalation of chlorine "scattered flowers on the borders of the grave?" Then Dr. Bennett himself admitted that he too had been using this "irritating," this "noxious" chlorine, which I am said to get from chloric acid, as a "*palliative*!" that is to say, for the purpose of *soothing* and *relieving* the irritation of the lungs. Then, regarding the absorption of the oxygen, the chemist said it would be absorbed by stramonium and the other agents with which it was exhibited. He could not, however, say whether the absorption would be slow or rapid, whether it would take *five minutes* or *five weeks*! As the mixture employed contained only two scruples of alcoholic spirit in a pint of boiling water, and an inhalation only lasts *fifteen minutes* altogether, his evidence on this point, to have been of any value, should have proved that the oxygen would be taken up in a much less period of time. When he says, "*with what rapidity I cannot say*," he shows us that he really knows nothing whatever of the matter from experience.

The answer to the whole matter is found in the fact that not one tittle of evidence was offered to show that *irritation* had resulted in a single instance treated by me.

Now, gentlemen, these are the views that are propounded by some of the most eminent men in the profession of medicine, and certainly by one of the most intelligent professors of chemistry that

it has been my good fortune to meet with. Probably you would think, after hearing all that, that the benefit resulting from the plaintiff's system of treatment may be problematical. If the *three* gentlemen among you twelve who are afflicted with incipient or advanced pulmonary consumption—because we are told that of every *four* people we meet, one is *consumptive*, either in the *incipient* or the *advanced stage*; though I am happy to say that I should have great difficulty in selecting the three from you, and therefore am able to congratulate you so far on being possibly an exception to the general rule—but I am inclined to think, after all we have heard, that if those three gentlemen, or the one-fourth of us, who may be under this terrible ban, were to resort to them, we should think there was not much comfort or hope to be derived from the promises which Dr. Hunter has in this work and these advertisements of his—perhaps quite honestly, but I cannot help thinking a little too rashly—put forward to the world.

A judge has, I suppose, a right to appeal to the passions of a jury; but if so, he ought at least to be perfectly sure that what he says, which may tend to prejudice their minds, is a fair and legitimate deduction from the facts before him. In this instance he stated that for which he had no grounds whatever. If one-fourth of the mortality from disease arises from one or other form of tubercular consumption, then one-fourth of the people may be said to be under the ban of that form of disease, and destined to fall a sacrifice to it sooner or later. Some will contract it this year, some five years hence, and others not for 20 or 30 years. In that period of time, which constitutes a generation, one-fourth will be found to have died.

When he told the Jury I had said that “*Of every four people we meet, one is consumptive, either in the incipient or the advanced stage,*” he told them what was calculated to prejudice their minds against me, and to make them believe that I had exaggerated the prevalence of the disease dishonestly. I have no doubt his Lordship made the statement he did, supposing it to be correct, and that the Jury felt honestly indignant with me for assuming that *three* of them must be in consumption! Such a statement his Lordship must have known would be damaging to me, and might even prevent me from obtaining a verdict. As it was untrue in substance and in fact, I cannot help thinking he should have taken greater care not to misinterpret my words.

In doing it, he probably actually prevented me from obtaining that redress which I was entitled to from the laws of my country.

Gross mistakes of this character would not be noticed in an *advocate*, for it is, I suppose, within his province to make them designedly if he believes he could thereby influence the Jury to the advantage of his client, but, coming from a *Judge*, who is believed to have a sincere desire to sift truth from falsehood, it falls with a weight of influence *a thousandfold increased*, and may not only blast professional character, but wreck the promise of life. As the Lord Chief Justice of England did make this mistake in my case, and did make it the basis of a *direct appeal* to the Jury, I cannot help feeling that I am entitled to redress for the wrong done me thereby. Where is the redress for this wrong? Who shall restore to me that of which I have been unfairly despoiled by his words?

But it does not at all follow that because Dr. Hunter's theory may be untenable—because his views, when they come to be examined by the light of science and of knowledge, may be proved to be erroneous—that therefore the defendant has been justified in saying of him all that he said. It is not because a man puts forward views upon medicine or upon any other science which prove, when they come to be severely criticised by competent persons, to be erroneous, that therefore he is to be held up to the detestation and scorn of the world. If every man who has advanced mistaken views, which science and reasoning and experience have afterwards proved to be delusive, were to be held up as a man wicked and sinister in design, there are a great many who still stand high in the world who would have to descend from the position which they occupy, and who might be pointed at with the finger of scorn as impostors. But that is not so. Unquestionably, when a man puts forward views upon any matter, to whatever department of human knowledge or human interest it may relate, he challenges criticism; and a man who differs from him, and who thinks his views delusive or mischievous, is perfectly warranted in dealing with his facts and with his reasoning with all the severity of an austere critic. He is entitled if he pleases to hold up the views to ridicule, but he has no right to impugn the motives, he has no right to say that the views, which may only be mistaken, are intended to carry out the purposes of wickedness or fraud, unless there is something that justifies that imputation. Hence it is not because these scientific gentlemen have satisfied you that there is no real substance or foundation in the views of the plaintiff that therefore he is to be held up as an impostor,

a swindler, and a scoundrel, unless you think that all the circumstances justify that portion of the charge.

Therefore, on the grounds of the theory and the practical application of that theory, I was entitled to redress. The Court should not have gone into an investigation which had no direct bearing on the issue to be tried, and concerning which it was not competent to form an opinion. There is not a theory in Medical science which could not, by the same kind of evidence—the carefully-selected evidence of those who did not believe in it, or were directly and pecuniarily interested in destroying public confidence in its truth—be overturned. What chance of justice would a Homœopathic practitioner have, if his medical theories and remedial means were to be fixed by the testimony of Allopathic practitioners? or a Hydropathist by either of these? The spirit of the law is revealed by the fact that no person is to be denied registration because of any particular theory of medicine or mode of practice. Then no man ought to be prejudiced by a judicial investigation into his theories, for no legal tribunal is competent to determine the value of scientific evidence when applied to rival systems.

Let us now pass to his consideration of motives.

Now, in order to see whether, besides criticising his views, the defendant is justified in impugning his motives, it is necessary to look a little more closely into the character of this book. Let us see what are the charges. I must just advert to them again for one moment. It is said by the defendant, partly through his counsel and partly by the evidence which he has adduced, that the plaintiff's system is not only utterly untenable, utterly fallacious, utterly delusive, but that he has put it forward with a fraudulent purpose; and that he begins, with great dexterity, by working upon the fears of those whom he addresses, and by dwelling emphatically upon the disastrous and fatal consequences of consumption. He tells that one-third of the deaths that occur annually in this country are occasioned by pulmonary complaints. He tells us that of the four persons whom we meet next in the street, one will be the victim of either incipient or advanced consumption; and that is put forward, is dwelt upon on the part of the defendant, as tending to show that the plaintiff intended to work upon the fears of those who might read his book or his advertisements. I am not quite sure that that is quite a fair way of putting it. It may have had that object; but I can quite understand that a man deeply

impressed with the importance of his subject, and writing a work upon it, by way of interesting those into whose hands his work or his advertisements might fall, of inducing them to attend to that which he was anxious to impress upon their minds, might at the very outset very well touch upon and dwell upon the fatal consequences of the disease with reference to which his work was about to be written. Therefore I do not think there is very much in that.

Although his Lordship does not think there is much in this, still it may be as well for the reader to carry in his mind the fact that I have already repudiated, as an absurd misinterpretation of my words, the imputation that I have ever said or written, that one-fourth of the people of this country were *affected* with consumption in any form; and especially as it will be found subsequently that his Lordship again and again recurs to the matter as a strong corroborative proof of other imputations. With reference to one-third of the deaths which arise from disease, I need only refer to the statistics to show that 134,264 deaths were registered in England and Wales during 1864 from pulmonary diseases, without including those registered under other names, known to depend upon tuberculous blood, and generally connected with tubercles in the lungs. Pulmonary diseases, therefore, make up nearly one-third of the gross mortality, which, according to the last-published report, reaches 495,531, which number includes accidents, violence, still-born children, and those dying at, or immediately subsequent to, birth. All the latter ought clearly to be separated from the gross mortality in estimating the perils of disease.

Sir James Clark's estimate is even higher than mine. After stating, on the basis of the tables of Young and Woolcombe, that "consumption causes a fourth part of the deaths that occur from disease," he continues, "If then we add to consumption tuberculous disease of the glandular and nervous systems of the large joints of the spinal column, &c., and deduct the mortality which occurs during the first months of life, I shall probably be within the truth in stating that a third part of the mortality in this country arises from tubercular diseases."

Every word, therefore, which has been said on this subject, might as well have been omitted. What Sir J. Clark stated I

have no doubt he believed. I have yet to learn that he subsequently modified his views on this point. In another place, I have given some rather startling statistical facts, which I hope will not also be regarded as *terrorist* writing.

But then they say that he goes on to excite the fears of his readers, with a view to their becoming his patients, in this way: that he tells them that which as a medical man he must have known not to be true—namely, that impeded respiration is of itself the cause of tubercular consumption, and that anything in the shape of a cold may produce it. Catarrh, in its ordinary form of cold in the head, cold in the nostrils, sore throat, which is neither more nor less than cold falling upon the throat; laryngitis, which is inflammation by cold falling on the larynx; and lastly, bronchitis, which is an inflammation of the delicate air tubes—may produce it.

Now, to most of us, who catch colds two or three times every winter, and who with colds get sore throats, and sometimes affections of the larynx, and occasionally affections of the bronchials, it is rather startling to be told that you may from any one of these causes get tubercle in the lungs. The witnesses say that that representation is not only untrue in point of fact, but is so untrue that no medical man could by possibility believe in its truth; and therefore they say that the object of it was to induce every person who found himself with a cold—that is to say, every nervous, timid person who found himself labouring under any of these common complaints—to take alarm suddenly, and imagine that from such trivial causes might proceed the germ of the disease for which there is no cure, and of which the end is death. That is what it is said, on the part of the defendant, is the object and effect of this book and the advertisements in respect of all these diseases. And if you think that the plaintiff could not have believed when he wrote all this that such consequences would follow from such causes, if you think that his purpose and object was to frighten people into coming to him as the sole person who could cure them; then it goes a long way to show you, not only that the theory and system and treatment are unfounded and unavailing and delusive; but that the plaintiff in putting it forward did so with a sinister design and evil motive, at the expense of illusory hopes and at the expense of aggravated illness and still more impaired health, to put money in his own pocket. Then you are to say whether the article which describes him as an impostor and a scoundrel, if these facts are well founded and these inferences are justly drawn, goes one whit beyond truth in that which it asserts.

The monstrous injustice of the proposition that twelve jurors—not one of them having a particle of medical knowledge—were competent to decide whether Catarrh, granular Sore Throat,

and chronic Laryngitis may produce Consumption, ought to have struck his Lordship's mind. Then they were to decide these questions on the evidence of medical men carefully selected because of their antagonism to my views. Then, again, his Lordship very far overstated what the medical men proved. Not one of them testified that these diseases *would not* develop Consumption. All that they contended for was that, in a certain number of cases, there was antecedent *taint*, and in others that the blood had become degraded *before* the local disease occurred to develop tubercles, and hence, though it might be *a* cause, or *the exciting* cause, it was not the *sole* cause.

Having started with the proposition that imperfect respiration is the cause of consumption, the plaintiff in his book refers to catarrh as one of the causes of imperfect respiration, and mentions a form of it which is rare, but which certainly, in the terms in which he mentions it, is calculated to excite very considerable alarm in anybody who finds that he has a cold in the nose. He says—

“Æzena is only another name for a very bad form of chronic catarrh of the nose, occurring in an unhealthy constitution. When the health is injured and the vitality of the system broken down, all local diseases are liable to assume an aggravated character. Not unfrequently æzena is produced by a vitiated state of the blood. From whatever cause it arises, it is a most serious affection; for, if it be not arrested, it will assuredly go on to the destruction of the bones of the nose, and may end fatally.”

There is something very unpleasant, you know, in the notion of losing your nose; and certainly it would seem, inasmuch as this does not appear to have much to do with consumption, that this might have been omitted, or not put forward in quite so terrifying an aspect as that which it is made to assume. However, that is what he says about catarrh. He then deals with sore throat, and he tells us what rather startles one; he describes the means to which doctors usually resort, and then he says, “In defiance of such means it”—the catarrh, that is, the sore throat—“proceeds slowly but surely towards the lungs, and the unfortunate patient soon learns how short is the step “from a sore throat to confirmed consumption.” That may be honestly written, or it may be written for the purpose and with the object of producing undue alarm, of terrifying a person who finds he has got a sore throat which lasts a little longer than usual into the belief that so long as he remains in the hands of his ordinary medical attendant he will not get better; that the sore throat slowly but surely will go to the lungs, and that there is one very short step from sore throat to consumption.

Then we come to the case of elongated uvula; for getting beyond the throat you get to the uvula. And certainly one is a little startled

at being told that because the uvula is elongated and descends into the throat, which unfortunately it sometimes does, and produces a very disagreeable sensation, which most of us have experienced at times—that because your uvula is elongated you are rapidly on the way to decline. In page 11, in describing the way in which the uvula acts, he says, “When neglected it commonly ends in permanent injury to the lungs, and often develops tubercular consumption.”

Now, the physicians who have been called, and who are conversant with these complaints, say that the statement that sore throat will necessarily proceed downward, and cause tubercle in the lungs; or that elongated uvula, because it tickles your throat, and makes you cough, will therefore, in many instances, end in consumption, is an utter untruth and pure delusion. It is for you to say what you think about this, gentlemen—whether the experience of the men you have heard in the witness-box is such as, so far as Dr. Hunter’s experience is concerned, may not have been borne out by the cases that have come under his view. On the other hand, if you find such statements put forward as these, which are undoubtedly calculated to excite considerable alarm in the minds of persons who happen to be of timid and nervous temperament; and, being subject to these complaints, or getting them for the first time, read such statements as these in the printed columns of a newspaper, it is for you to say whether they are put forward under the honest conviction of their truth, or for the purpose of deceiving people into the belief that they are in danger of their lives, when in fact they labour under common complaints which any medical man can deal with.

Then, with regard to enlarged tonsils, which is a very common sort of complaint, you are told that enlarged tonsils, if you do not take care, may produce consumption, because he says, “What I wish to impress most particularly is that enlarged tonsils cannot be allowed to remain without endangering the health of the lungs.” Then we come next to the inflammation of the larynx—the upper part of the trachea or windpipe—which he says will descend to the bronchials, will cause the air-passages to be thickened and obstructed, and in like manner end in consumption.

If I have given a truthful account of the symptoms, nature, and tendency of these affections, then I shall look for that justice from my countrymen which their proverbial love of fair play entitles me to expect.

Here are the proofs:—

Professor GROSS says—

“Ulcers in the nose of a *strumous* nature are sufficiently common; and, from their rebellious character and fetid discharges, are often a source of great annoyance, both to the patient and the practitioner. Seated originally in the

"mucous membrane, they gradually extend in depth, until in many cases they involve all the component structures, cartilage and bone, as well as fibrous tissue. . . . In the scrofulous variety, one side alone may suffer. The discharge attendant on this disease is noted for the intensity of its fetor—whence the term *ozæna*, by which the affection is usually designated. In the more aggravated forms of the affection, large quantities of mepORIZED mucus pass off; or, collecting in the nasal cavities, form thick brownish incrustations, which drop off every fourth, fifth, or sixth day, only to be succeeded by another crop. Portions of cartilage and bone, or even entire bones, often die and slough away. . . . It should not be forgotten that a bloody and fetid discharge may proceed from the nose, in consequence merely of the presence of a foreign body—retained secretion, or disorder of the general health (page 415). . . . Thickening of the mucous membrane of the nose, observed chiefly in children and young persons of a weakly *scrofulous* constitution (p. 416). *Chronic* enlargement of the tonsils is exceedingly common, and is met with *almost exclusively in young scrofulous subjects*. The uvula, from debility, inflammation, and other causes, is liable to chronic enlargement, especially elongation. . . . It may occur at any period in life. . . . It is generally the result of repeated attacks of cold. . . . It is frequently conjoined with inflammation of the palate, tonsils, and fauces, with derangement of the digestive organs, and a *scrofulous* diathesis. The affected organ may project down into the rima of the glottis, occasioning *aphonia*, or a change in the tone and power of the voice. . . . The more common effects are obstinate protracted cough, with frequent desire to clear the throat. . . . When the affection continues long, TUBERCLES sometimes form in the lungs, and the patient ultimately *dies with all the symptoms of confirmed consumption*" (p. 576).

ANCELL says—

"In tuberculous subjects, the lining membrane of the nose is extremely irritable, and liable to copious, thin and acrid secretions, and to small erosions. It is extremely liable to chronic inflammatory action and muco-purulent discharges [catarrh], sometimes inducing either a diffuse or more circumscribed hypertrophy, and occasionally the formation of polypi, or ulcerations, which degenerate into *ozæna*. In these affections, the laminae of the bones are liable to become affected with caries. . . . Many cases of *ozæna* and diseases of the antrum and bones take their origin in *tubercular* deposits! Chronic inflammation of the membrane lining the nasal fossæ is a somewhat frequent affection. In one case I found the antrum loaded with concrete tuberculous pus."

DR. EDWARD SMITH says—

"The chief sources of secretion in the *early stages of consumption* are the fauces and pharynx. The tonsils emit a secretion of a glairy nature, &c." (page 137).

Again—

"In this condition there is not unusually, but yet not necessarily, some *elongation of the uvulæ*, and a little suffusion of the mucous membrane of the

‘ whole fauces, with enlarged vessels upon the back and sides, and sometimes “ also some *enlargement of the tonsils!*”

It is of no consequence whether these authors agree with me that these conditions precede and actually lead to the acquisition of consumption, or assume that they precede the development, but depend on some antecedent predisposition, which they have merely served to develop. My view is certainly the least alarming. In describing them as forerunners of consumption, and as often *connected with it*, I have done no more than every physician is compelled to do, if he would give a full and true account of the cognates of consumption. If it be assumed that “*ozœna*,” “*follicular sore throat*,” “*elongated uvula*,” and “*enlarged tonsils*,” are generally or often dependent on the *scrofulous* predisposition, then the importance of early treatment becomes greatly enhanced. If consumption is to be prevented, all affections which directly tend to its development must be promptly treated. If neglected, they certainly do generally end in consumption. This is all I have said in my book. If I had said less, I should neither have expressed my own convictions, nor regarded the experience of contemporary authority.

Then, after dealing with laryngitis, he comes to the most serious of all affections, bronchitis, and there he certainly does throw out a very dismal picture to those who believe in the reality and truth of his doctrine. He says—

“Bronchitis, then, is a disease of the lining of the bronchial tubes ramifying the substance of the lungs. This lining or membrane is very sensitive to every external influence, and is hence liable to become irritated by sudden changes of weather, by impure air, by the fine particles of matter which fill the atmosphere of workshops, or float upon the wind in dusty streets, by the hundred irritating causes which meet our breathing in almost every place. The most common cause, however, is a cold, showing itself first as a catarrh of the nose, next affecting the throat and larynx, and lastly involving the bronchial tubes, when the disease is known as a ‘seated cold in the chest,’ or bronchitis. The acute symptoms usually subside after a short time; but unless the patient is properly treated, he does not return to his former good health. He discovers a slight disposition to warmth in the hands towards evening, and cannot take the same exercise as usual without discovering that he is ‘short of breath.’ Still he is pretty well, and if the season be summer, he may go on until fall without expectoration. But as the winter approaches, he begins to cough, and the expectoration is found to be yellow. He may also find the fever increased to decided hectic, with night sweats and rapid loss of flesh, in which case he will probably die before the spring, with all the symptoms of consumption. Still this is not consumption at all; it is simply a chronic catarrh of the lungs. It is very common for chronic bronchitis to assume a milder form, when it is

"spoken of as a 'winter cough.' It comes on each winter, and as regularly subsides during the summer; but at every succeeding recurrence it manifests itself in greater severity, and the recovery in the following summer is not so complete. This form of bronchitis, if neglected, will as surely destroy life (and treated in the usual manner it is as incurable) as consumption itself."

If you get bronchitis one winter, your ordinary doctor will stop it for a time, but it will come on the next winter. You will get better towards the summer, but will become bad again in the winter. The next year it will be worse still; you will still go on with the ordinary remedies, but they cannot cure it, and you may just as well be in a consumption, for it will end in killing you. Then he goes on to say—

"The mucous membrane sooner or later becomes altered in structure, and pours forth a matter which has all the qualities of pus. Hectic fever supervenes, and the disease tends slowly to a fatal termination."

I am gratified at having this connected description of a diseased condition from my work, especially as it was held up by his Lordship before the eyes of the Jury as a very strong point against me. Indeed, it was for them to say whether such *false* terrorist writings did not justify all that the *Pall Mall Gazette* had said, and whether I was not that "scoundrel" I had been represented. Of course I had to sit and listen to all this, for the Lord Chief Justice of England was addressing the Jury, and vindicating the wisdom and impartiality of British justice! Now let me lay before you a few extracts from the work of the most eminent physician of England.

Sir THOMAS WATSON, speaking of this disease, says—

"Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort" (p. 27).

Again—

"Catarrh is very often met with in a chronic form. In other words, the mucous membrane of the air passages is very liable to be affected with chronic inflammation. Chronic catarrh is often a sequel of acute Bronchitis; and it is a form of complaint that is full of interest on this account, if on no other, that it has so often been mistaken, and is so liable to be mistaken still, for tubercular Consumption; of which, indeed, it is very frequently the companion."

"The *constant* symptoms of chronic Catarrh, or Bronchitis, are cough, some shortness of breath, expectoration of altered mucus. The *variable* symptoms, those which are oftentimes of the most importance as determining the slight or the serious character of the disorder, consist in the quantity and quality of

"the matter expectorated, and the presence or absence of wasting, and of hectic fever.

"You will continually be meeting with cases of *this* kind. A person advanced in years has what he calls a slight cold in the winter. He coughs and expectorates a certain quantity of grey or transparent mucus. In the summer his cough diminishes or ceases altogether. The next winter the same thing happens again, and each successive return of the colder seasons of the year brings back in increasing severity the cough and the expectoration."

"But chronic Bronchitis may take place at any age, as a sequel to acute; just as active inflammation of other parts of the body is liable to degenerate into the chronic form; and such cases are sometimes very equivocal and deceptive."

"In truth, chronic Bronchitis is, in some cases, as incapable of recovery and as surely and progressively fatal as tubercular Phthisis itself, and even more so than some of the forms of Phthisis" (p. 52).

Again at page 54

"When the membrane and the tubes which it lines become *altered in structure*, and pour forth a fluid which has all the qualities of pus, hectic fever generally is present, and the chronic disease tends, slowly perhaps, but surely to death."

Will any impartial person pretend to say that my description is in any sense different from Sir Thomas Watson's? And if not, how am I to obtain redress for the outrage put upon me?

Now hear what LAENNEC says on the same subject:—

"Acute pulmonary catarrh [bronchitis] is commonly preceded by a cold in the head, a perfectly similar affection of the lining membrane of the nose. After some days or some hours . . . the inflammation reaches the bronchial mucous membrane; its passage to the larynx is denoted by a sense of irritation or titillation, which provokes coughing (p. 63.) . . . In the greater number of colds, the symptoms of pulmonary catarrh [bronchitis] (p. 64), chronic mucous catarrh [bronchitis] generally succeed a severe acute attack. The fever ceases without the cough, and expectoration becomes less; or it changes into a slow fever, which is scarcely felt, except towards evening or in the exacerbations of the catarrh. The patient regains his appetite and strength, but remains, commonly, paler and thinner than before the attack. In a state of repose he experiences no difficulty of breathing, but is easily put out of breath by exercise . . . The cough and expectoration diminish or even disappear in *summer*; but even then the patient retains a dry latent catarrh, the nature of which will be presently described. In *winter* the catarrh again becomes mucous and manifest, and its return is often attended with fever, especially when the expectoration is copious. . . . *Hectic fever*, in some rare cases, is established; *emaciation*, which in this disease is usually moderate, *rapidly increases*, and the disease ends in *death*, after presenting symptoms so like those of *Consumption*, that we have as yet no possible means

"of distinguishing them, except by autopsy [examination of the body after "death]. In fact, the most perfect resemblance exists between the two diseases, "in respect of the *sputa*, the *emaciation*, and all the other symptoms."

Where is the difference between his description and mine? If there is substantially none, then both *truth* and justice have been outraged by the medical evidence of the defence, and his Lordship's remarks based upon it. But let us attend his Lordship further, and you will learn what other errors of description I am guilty of.

Then he talks of bronchitis being "peculiar to middle life and old "age." Next he says—

"The symptoms of dry bronchitis are not marked by severity. It is the most "insidious of pulmonary complaints. The subject of it is conscious only of "being short-breathed when ascending an elevation or attempting to run. "When a large portion of the lung is involved, a sense of oppression is experienced after meals and on very slight exertion. By some this oppression is "referred to the opposite side of the chest to that on which the disease is "situated, or to a remote part of the same side, or even the region of the "stomach."

With reference to my description of Bronchitis, it so closely resembles the admirable description of that disease given by the great Laennec, that it is impossible to read his account and not be struck with the similarity.

LAENNEC says—

"Dry catarrh [bronchitis] often remains moderate in degree and perfectly "latent for a long series of years. Those affected with it only perceive that their "breathing is a little shorter than that of others when they try to run or ascend "a height. When the bronchial congestion has reached a certain extent, "dyspnœa [shortness of breath] is felt even in a state of rest, and especially "after a meal. Some patients refer the sensation to but one side of the chest, "and sometimes to that least affected" (p. 79).

Then he says that it gets worse, and worse, and worse, and then that the patients, if asked whether they have a cough, "will, almost without exception, answer, 'No;' yet, during your conversation, they "will perhaps *hack*, and raise a little jelly-like mucus half a dozen "times. At intervals the cough is more severe, and comes on in "paroxysms, when too commonly it is regarded as simply *nervous*." He next deals with the mode in which the profession deal with these symptoms.

Here again his Lordship holds up that as a reproach which is mere matter of description, and inseparable from a true account of the disease under consideration.

LAENNEC says—

“The quantity of sputa is often so inconsiderable that the patients themselves do not perceive that they expectorate and cough; in others, there is actually neither cough nor expectoration; and in many there is only a trifling cough, which is perfectly dry, and sometimes so infrequent as not to occur more than once in the day, or even in two or three days. This cough, when the catarrh has come on slowly, and without being preceded by acute bronchitis, is by the majority of practitioners called a *nervous* cough.”

“If the stomach be at all deranged, it is fashionable to consider it a mere ‘stomach cough,’ or as being caused by ‘liver disease,’ while, in fact, the derangement of the stomach, liver, &c., which exist, are merely consequences of this very condition of the lungs. There is something so gratifying in being assured by the family physician that there is ‘no danger of consumption,’ that ‘this cough is a mere trifle and will speedily pass away if only left to itself,’ that it is not surprising the flattered patient entirely overlooks the important fact that no careful examination of the lungs has been made to determine the health or disease of this organ; that this gratifying opinion, therefore, is founded on nothing reliable, and, reduced to its intrinsic merits, is simply tampering with human life. Most ‘coughs of long standing,’ unattended by free expectoration, are caused either by this condition of the bronchial tubes, or by incipient tubercles in the lungs. The ‘neglected cold,’ which so often proves the herald of consumption, is but another variety of this disease.”

I take the same exception to his Lordship’s remarks on this point. There is nothing in my description which calls for criticism, unless to proclaim the truth be a crime, and to suppress it a virtue.

LAENNEC says—

“The frequency of dry bronchitis, the *insidious* slowness of its progress, and the serious nature of its effects, particularly when it has become intense, ought to impress us with a sense of the necessity of not treating long-continued dry coughs with indifference, however infrequent and slight they may be. These coughs, which are too often regarded as *nervous*, stomach, liver, &c., and referred to a *sympathy* unknown, both in its nature and mode of action, and often most gratuitously assumed, are really nothing but the effects of dry catarrh” (page 91).

Now, therefore, gentlemen, we arrive at the last stage before we come to the pulmonary consumption itself. We have passed from catarrh of the head to catarrh of the nose, from that to sore throat, from sore throat to laryngitis, from laryngitis to bronchitis, then we find that bronchitis occasions the fatal thickening of the delicate air-tubes, and that that causes an obstruction to the passage of the air; then that the necessary quantity of oxygen does not come into the lungs, that a

carbonaceous deposit takes place, that tubercle forms, and very soon that there is an end of the unhappy patient.

From his Lordship's sneering observation regarding *thickening* of the Bronchial tubes, I am led to suppose he doubted that it existed as a consequence of chronic disease of the mucous lining of the air-tubes. If it will be easy to disabuse his mind,

Dr. W. H. WALSHE says—

"Narrowing and obliteration of the bronchia, not an uncommon phenomenon in tubes of very small calibre, becomes rarer and rarer in the direct ratio of their increase of width; still, obliteration of the main trunk has occasionally been witnessed" (p. 233).

Again—

"The obstruction [of the bronchial tubes] may depend on intrinsic or extrinsic causes. Among the former rank thickening of the mucous membrane; stagnation of secretions, especially of the plastic kind . . . great thickening of the bronchial walls with induration matter or fibro-plastic substance; and, lastly, accumulation of cancer or tubercle in their interior, &c." (p. 234).

Sir THOMAS WATSON also says—

"Thickening of the mucons tissue [of the bronchi] occurs also in various degrees, and in connection with the various morbid conditions of this membrane," &c. (p. 63, vol. ii.)

Dr. CHARLES J. B. WILLIAMS says—

"The most simple change of structure is a mere thickening of the mucous and submucous membranes (p. 94). Nothing is more common than to see the air-tubes of persons who have long suffered from bronchitis, presenting an undue development," &c. &c. (p. 95).

The point may not be of much consequence, but it serves to illustrate the false grounds on which everything pertaining to this case rests, and the extent and character of the injustice which has been done me by basing upon a *fiction* false and degrading imputations against my professional character.

Now, looking at all those representations of the plaintiff, the manner in which they are put forward, and the language in which they are put forward, in your judgment are they put forward with an honest desire of warning those who may be subject to these complaints—whether they get them in the first instance, or become subject to them from long habit—is the object to warn such persons, and to rouse them from an apathy as to their condition to a real sense of their danger—is it for the honest purpose of inducing people to adopt the

honest belief of the writer that, independently of any hereditary or acquired disposition to tubercular disease, these colds in these various forms will produce the thickening of the air-passages, and by that eventually produce a diseased condition of the lungs? Or is all this put forward in the particular mode which the writer adopts, for the purpose of frightening people (who, on the contrary, so far from being frightened, ought to be encouraged and induced to look hopefully on their condition) into a belief that they are in a desperate state, from which alone Dr. Hunter can relieve them?

Let us now see what others not interested in perverting facts say in regard to *Catarrh*, *Sore Throat*, *Laryngitis*, and *Bronchitis*, as causes of Consumption.

CATARRH.

Professor HUGHES BENNETT published, in 1859, a work on Pulmonary Consumption, in which he tells us—

“He has pointed out what he believes no writer has hitherto noticed, viz., the importance in some cases of examining and treating locally diseases of the nasal passages.” (p. 7.) “Again, I have seen numerous instances of chronic coughs . . . which have recovered by discovering that the disease originated in the nasal passages, and by directing a treatment to them (p. 217). “Lastly, in the following case of incipient consumption, I was enabled, by treating the nose, to produce recovery, when there were no indications of lesion in “the nasal passages.” (p. 219).

SORE THROAT.

Dr. EDWARD SMITH says—

“We pointed out that there is ordinarily a condition of the throat in phthisis quite different from that which is found in Chronic Bronchitis.” (p. 124). Having described one form of disease, he proceeds to a *second*. “The condition now known as follicular disease, in which the simple epithelial glands, which are abundant on the posterior wall of the pharynx, become enlarged and vascular, and present the appearance of large flattened currants.” (p. 126). This second form is the one described by me under the name of ‘*granular sore throat*.’

LARYNGITIS.

Sir JAMES CLARK says—

“Ulceration of the larynx is a frequent concomitant of tubercular disease of the lungs . . . occasionally symptoms indicative of its existence make their appearance before the signs of the pulmonary affection are very evident.” (Cyclop. of Prac. Med. Am. Edit., p. 521).

Louis “found ulceration of the larynx in *one-fourth* of his cases of consumption.” (Ansell, p. 173).

Professor SWETT says—

"*Chronic Laryngitis* is, in a great majority of cases, a condition of the "scrofulous diathesis. In most instances, indeed, it is associated with manifest and advanced tuberculous diseases of the lungs. In other cases, the evidences of pulmonary disease are not so well marked, or there is no evidence of their existence until, in the progress of the case, they begin to appear."

"When you see a case of chronic laryngitis, the idea that tubercles exist in the lungs should at once suggest itself to your minds. Even if evidences of their existence are not clear, their presence may be suspected, or at least "feared." (Lectures, p. 165).

BRONCHITIS.

ANCELL tells us that—

"Bronchitis is regarded by a great number of authors as a cause of consumption. Herfeland, Tissot, Baumes, Broussais, and, I believe, all the disciples of his school, and many practitioners in this country antecedent to the publication of Sir James Clark's work, held that opinion."

Dr. C. J. B. WILLIAMS says—

"In the first class of cases, we have development of consumption from local inflammation, without any evidence of the prior constitutional disorder. . . . Under any of these circumstances, *chronic* inflammation, either by its own local effects, or by its depressing influence on the constitution, or by both combined, becomes a sufficient cause of pulmonary consumption" (p. 172.)

Some of these authorities, I am perfectly aware, assume that there must be some antecedent taint in the constitution; but that is something which they cannot know and cannot prove, hence it is only assumption. It is enough to know that many high authorities do believe with me, that *impaired respiration*, caused by local inflammatory affections, may and does lay the foundation of Consumption. Some even contend that chronic *Catarrh*, follicular *Sore Throat*, and ulceration of the Larynx are themselves evidences of the strumous diathesis.

Professor Gross is strongly of this opinion. If we admit this, then there is all the more danger of the pulmonary disease being set up by the continued irritation and interference with respiration. No person having any experience in the treatment of pulmonary affections can doubt the ultimate connection that exists between polypus of the nose and Asthma. The views which Professor Bennett, of the Edinburgh University, advanced in 1859, in regard to the connection between Catarrh,

Coughs, and Consumption, I published in 1852 in New York. If I am a "scoundrel" for holding these opinions, then all I can say is, that I am in very good company.

What is the position which the plaintiff assumed? That not only consumption or tubercular deposit in the lungs, but its cognate diseases—those which lead up to consumption, and, according to his theory, are almost a certain cause of it—are all improperly treated at the present time, and in a manner not calculated to insure their removal. Not only so, but when you come to consumption itself, the disease is admitted to be incurable. It is tampered with by medicines administered through the stomach—medicines which cannot reach it. Patients are dosed with physic and they are filled with cod-liver oil that is of no avail.¹ "There is but one remedy, and that remedy is "inhalation. For that you must come to me, and I will cure you."²

¹ I have not written as strongly in condemnation of a reliance upon cod-liver oil and tonics alone, as many others have done. If it be denied to medical men to discuss in a temperate spirit their views, then there is an end of progress.

I have already shown by reference to my book, p. 102, that the profession admit the curability, but also confess their inability to point out a satisfactory mode of treatment, whereby cure may be accomplished. The "*curability*" of a disease is proved by one well-marked case; but how to render cure the rule, and failure the exception, is what is desired. With all the advantages of a regulated diet, strict attention to hygiene, and the aid of cod-liver oil and stomachic medicines, the results attained at the Brompton Consumption Hospital are so unsatisfactory, that Dr. Edward Smith, one of its staff of physicians, tells us that "*the disease is still essentially and almost as universally a fatal one as it has been in all ages*" (p. 30).

² This assumption, which I see in quotation marks in the *Pall Mall Gazette*, "Come to me and I will cure you," should, I think, be credited to the source whence it emanated. It is certainly not to be found in anything I ever wrote. It was one of the numerous facetiæ of the Lord Chief Justice, and I have no wish to deprive him of the credit of the invention.

There is another part of the work to which, with the view of enabling you to form a judgment as to the motives of the plaintiff, I think I ought to call your attention. It escaped my recollection in

its proper place, and I will say what I have to say upon it now. The plaintiff, in his work, does what one would expect—he gives a detail of the symptoms by which consumption may be known to the person afflicted. This again, it is said, is an instance of his working upon the fears of people, and inducing persons to think themselves afflicted with consumption when they are not, and most certainly it must be acknowledged that some of these sentences do sound a little oddly in one's ears. Says he, if you have a hacking cough, if you have shortness of breath, if your pulse is accelerated ten or fifteen beats beyond what was its normal pulsation, these are infallible signs of consumption.

Here again the Lord Chief Justice failed to quote me correctly, and he added words which materially change the sense intended. "*Infallible signs of consumption*" are not my words, but his! I had described, as early symptoms of the disease, "*a dry hacking cough,*" "*shortness of breath,*" and "*increased frequency of the pulse,*" and, in closing the chapter, I summed up in the following words:—

"If, *therefore*, you have a hacking cough, and slight shortness of breath on exertion, *accompanied* by an increase in the frequency of the pulse of from 10 to 15 beats per minute, you cannot doubt the existence of mischief in the lungs, and should instantly set about its removal" (p. 44).

There is nothing in this about "*infallible signs of consumption.*" Besides, I had explained, in what went before, that these symptoms, separately, were not of much consequence, but, taken together, indicated that, from some cause, the function of the lung was not properly performed; and, as *prevention* is better than the chance of *cure*, especially when the disease is consumption, the patient ought to set about the removal of whatever might, on examination, be found to exist. Why, let me inquire, did his Lordship state my views to the Jury in *his own words*, when he had my book before him, and might have used *mine*? If he might change the word "mischief" into "infallible signs of consumption," there is scarcely any conceivable modification of language which might not be made with equal propriety.

Now, a good many of us are troubled sometimes with a hacking cough.

A Juror: I think, my lord, it is presuming that some of us are not

very sensible. It presumes that we are all fools, or something similar to it.

The Lord Chief Justice: Then, with regard to the shortness of breath: I do not know, gentlemen, whether some of you are, like myself, getting on in the vale of years, but I do not find that I can walk up the side of a hill as I used to do, because my breath will not come so freely and actively as it did. Then, with regard to an accelerated pulse, why if you have a bad cold your pulse goes up, and from an infinite variety of causes an acceleration of the pulse will be caused. Is it right, is it truthful, or is it honest to put forward to the public that, if you have a hacking cough, or a shortness of breath, or an accelerated pulse, to the extent of ten or fifteen beats beyond the normal condition, you must therefore necessarily be in a consumption?

His Lordship, in the above paragraph, entirely falsifies my language and meaning. I said a hacking cough *and* shortness of breath, *accompanied by* an increased frequency of the pulse. The three symptoms were united together, and it was only when they continued to exist without appreciable cause that they were said by me to indicate mischief of some kind in the chest. He drops the copulative "*and*," and substitutes the disjunctive "*or*," which makes it read as follows:—"If you have a hacking cough "you are in Consumption, or if you have a shortness of breath "you are likewise in Consumption, or if your pulse is accelerated "ten or fifteen beats beyond the normal condition, that also indicates "Consumption." This is the way he placed the matter before the Jury! This, however, is his Lordship's teaching, not mine. Even the term "*Consumption*" is an addition by him. If the Jury believed it to be my language, and decided against me because of that belief, then was I *robbed* of justice by the palpable and gross misconstruction of my language by the Lord Chief Justice of England.

Now let us examine the material fact.

Sir JAMES CLARK says of *cough*—

"This is the first symptom which claims our attention. . . . During the first "weeks or months, it is usually a *slight dry cough*—occurring chiefly in the "mornings, on the patient's getting up, or making any bodily exertion during the "day—to this state it is scarcely *noticed* by the patient."

Again—

"We have never known the cough entirely wanting; but have known it so "slight, that it has failed to attract even sufficient attention to alarm a very "nervous patient."

And further—

“When, from the continuance of the cough, or its doubtful character, one suspects some cause beyond catarrh, we should inquire carefully into the patient’s state.”

If a patient has only *one* of the three symptoms which I linked together, Sir James thinks we ought to inquire carefully into the state of the chest—in other words, that we ought to be on our guard lest consumption should be at the root of the matter, and get a firm hold before it was detected. This would be the advice of every careful, conscientious physician.

Speaking of the pulse, Sir James says—

“A frequent pulse, even taken as an isolated symptom, should *excite suspicion*; and, when accompanied with symptoms indicative of tuberculous disease, it strongly favours the presumption that *mischief* has already commenced” (p. 99).

Now, as I entertain the highest possible respect for Sir James Clark as an authority on consumption, and have always regarded his precepts as a safe guide in descriptions of disease, I am at a loss to see why, if he may say with propriety, “*a frequent pulse, even as an isolated symptom, should excite suspicion,*” I may not say that “*a frequent pulse, with the addition of cough and shortness of breath, should excite suspicion; and that it is at least prudent to get rid of them.*” These sentences may “sound a little oddly” to the Lord Chief Justice, because he knows so little about medicine. But is that not another reason why a dispute among rival doctors, and irrelevant questions of theory and doctrine, should not have been imported into a civil action for libel, and decided by a jury even worse informed on the subject than himself?

But then there is another thing; he says that losing flesh is a sign of consumption; so is gaining flesh. You sometimes see nice, rosy, plump-looking young girls the very picture of health, but he deals with them the same way; that is nothing to the purpose; they have consumption. Especially if you change now and then, if you add to your weight at one time and lose it at another, it is consumption—consumption! It is really for you, gentlemen, to say whether you believe these things are put forward in an honest and truthful spirit, for the mere purpose of inviting the profession to consider them and to benefit mankind, or whether they are put forward with the sinister design and purpose suggested by the defendant.

His Lordship here indulged in two assumptions, both of which are at variance with facts. He assumed that *loss of flesh* was *not* an important and significant symptom in the early stage of Consumption. It must be understood that to the explanation of what is meant by the term "loss of flesh," I had devoted more than a page of my book. It was loss of flesh "*while the appetite remained good, and the food abundant and nutritious.*" It was *slow, gradual* loss of flesh, with alternate gain and loss, "without any appreciable reason" for it, that I pointed out as suspicious and deserving of attention (p. 46). I then coupled this symptom with others in the following sentence:—

"If, *with* loss of weight, there is a disposition to sigh, a *dark discolouration beneath the eyes, and a quickened pulse, with some heat in the hands,* set it down as "almost certain that the lungs are affected."

I had previously said—

"*We do not always find apparent* loss of flesh in the *first stage of tubercle.* In "young women particularly I have often found the lungs seriously affected, "*while they still retained their colour and plumpness*" (p. 47).

Now the question is not whether these sentences sounded oddly to the Lord Chief Justice, but whether they were true. If he had turned to any reliable work on Consumption, he would have found that they were unquestionably true, and very fairly stated. Since he admitted evidence on these points at all, he ought certainly to have been perfectly sure, before suggesting the possibility that they had been put forward with some "sinister design," that they were not a truthful description of what really takes place.

Sir JAMES CLARK says—

"In some cases the wasting is one of the first circumstances which attract the attention of the patient's friends. In others, the disease makes *considerable progress* before the patient becomes *visibly* thinner; examples of which we have found most frequently in *young females!* . . . As a symptom of tubercular disease, emaciation merits special attention in obscure cases. In persons about the middle period of life, from forty to fifty, we have found it one of the *earliest* symptoms, even when there was *no* frequency of pulse, *no* cough, *no* marked shortness of breath, or other symptom to draw attention to the lungs. . . . Emaciation should never be disregarded, when there is no evident cause for its presence."

DR. COTTON (one of the defendant's scientific witnesses) says in his book, p. 100—

“ Loss of weight is of *invariable* occurrence; sometimes the decrease is so rapid “ that it will attract the attention of friends; at others, it requires the periodical “ use of the weighing machine to detect it.”

And this, too, is before tubercles are deposited in the lungs.

ANCELL says—

“ Notwithstanding Louis came to the conclusion that in one-half of the cases of “ Consumption observed by him, *wasting* existed among the earliest symptoms; “ and that Fournet found tuberculous subjects frequently *moderately fat*, being “ apparently in a state of perfect *embonpoint* before any disease set in, and occa- “ sionally very fat—to such subjects, the majority of whom *were women*. . . . “ Emaciation is one of the most general structural changes in consumption “ (p. 66). . . . Dr. Hutchinson lays it down that a *slow* and *gradual* loss of weight “ is more serious than a quick and irregular loss. . . . A person may from time “ to time *lose* and *gain* weight, but he cannot do this slowly and gradually without “ exciting suspicion of the existence of serious disease ” (p. 69).

Dr. W. H. WALSHE, Consulting Physician to the Hospital for Consumption, says—

“ In some instances Consumption runs its course almost to the end without “ notable emaciation; the body may be *plumply fat*, while large cavities exist at “ the apices, and the disease makes rapid advance downwards” (p. 461).

So much for these *two symptoms*. I have taken greater pains in showing that in every essential my description was strictly accurate, because it was a subject of remark that, during the trial, his Lordship frequently adverted to the “ *young women*,” and seemed extremely anxious to discover whether they could “ pre- “ serve their colour and plumpness” in this disease. All this, no doubt, arose from feelings of humanity towards the young women, mingled with a great love of science, which naturally made him desirous of penetrating its profoundest mysteries. But what I cannot understand is, *why*, after he had been informed that they *do* sometimes preserve their colour and plumpness, he should have thought it necessary to allude to the matter further, or why, in his charge to the Jury, “ *nice, rosy, “ plump-looking girls*” should have rolled from his tongue in all their *borrowed* glory! “ *Nice, rosy, plump-looking girls*” are all very well, no doubt, but they are not in my book. I hope his

Lordship did not mean to put them there with all these *additions* and *variations*!

“It is really for you to say, gentlemen, whether you believe “these things are put forward in an honest and truthful “spirit!” Such was the language addressed by his Lordship to the Jury, and it is also the language in which I now address myself to the people of England. It is for you to say, gentlemen, whether these *fictions* of the *Advocate* are not incompatible with the *functions* of the *Judge*. It is for you to say whether exhibitions of this character from the Bench do not unfairly *incline* the beam of justice, and endanger the rights and liberties of every subject of the realm. I am but one, yet if this thing may be done to me with impunity a *precedent* is established which becomes applicable to you, one and all, and no man who has justified that precedent in my case can afterwards complain of its application in his. In the name of IMPARTIAL justice I enter an indignant protest.

But now I must come back to the point that I was upon—what is the motive of all this, is it to promote his own interest or what? Undoubtedly the tenor of the book is to show that it is useless to go to the ordinary medical practitioner; no matter what confidence you may have in your medical man, although he has treated you for years, and carried you safely through such ailments as you may have unfortunately been exposed to; although as a medical man he may stand in the very first rank of his profession, may have obtained all its honours and rewards, and the public have perfect confidence in him, if you happen to have anything the matter with your lungs or other organs, do not go to him, he will give you a quantity of nauseous stuff, a quantity of that abominable cod-liver oil, but he will do you no good; and, what is more, he will delude you with false hopes, that will only end in your destruction. He is your family physician, and has a great regard for you, and does not like to make you uncomfortable or to see your friends and relatives and those about you unhappy; he tells you that is only a little hacking cough, or a liver cough, or a stomach cough, and that you must wait till the spring and then you will be quite well.

But, says the plaintiff, that is tampering with human life; do not go to him; the profession can do nothing with this fatal disease; in their own hearts they know it; they may delude you with false hopes and expectations, but you will never see the spring, you will be in your grave before it comes; and they in their own hearts and minds know that they cannot do you any good. Who can? Why the man

who administers—not medicine, not cod-liver oil, through the stomach, but the man who administers remedies by way of inhalation. Who does that? The ordinary physicians? No. They would not acknowledge its efficacy; they are too apathetic to adopt its use; it does not square with the ordinary routine of professional treatment; they will not have anything to do with it. They either do not appreciate it or they will not use it in practice; besides, it is only a man who has got one of the necessary instruments that can do it, and who has faith in the system. Where is the patient to go to? There is but one quarter to which this book points, and that is to Dr. Hunter himself. Is it possible to read this book, with all the remarks contained in it upon the medical profession, without coming to the conclusion that what he means is this?—Abandon the ordinary remedial agents to which persons in cases of these disorders usually resort, give up the ordinary practitioners, because they are altogether in the wrong road, and they cannot lead you to any happy or good result. Give them all up; and if you have anything the matter with you, though it be only a cold in the head, come to me. Now that I take to be the effect of the whole book, and it must be for you to say whether, looking upon it as a whole, you are satisfied that the system which he propounds is based in error and in delusion; and then if you are of opinion, looking at the whole of the matter, looking at the way in which he is alleged to have worked upon the fears of those into whose hands the book falls, looking to the fact that he calls upon them to address themselves to him for the only remedy—if you are of opinion that he, as a medical man, must have known that his remedy was delusive, if, judging from all the circumstances, you arrive at that conclusion—you will probably think that so far as relates, at all events, to his treatment of consumption, the terms of the article, strong as they may have been, were justified and warranted by all the facts of the case. But of course it is for you to exercise your judgment upon the facts.

I shall not take the trouble of commenting on more than two or three points in the foregoing. Much of it is based on the assumption that my descriptions of disease were different from those of other medical men; and certainly the astounding evidence given by the medical witnesses of the defence gave him a right to assume that their statements had more foundation in fact than was really the case. As I have shown that on each individual point, paragraph, and sentence referred to by the Chief Justice, he and the scientific witnesses were wrong and I right, all that is necessary to add is, that I regard his observations as a serious injustice perpetrated upon me, which nothing except his ignorance of medicine can excuse.

As I have said, it is not because a man in error propounds an untenable theory that therefore he is to have his motives impugned, and to have sinister and wicked designs imputed to him. We must take great care in judging a question like this that we do not fall into either of two extremes. It was put forward by the learned counsel for the plaintiff in his very able address, that this article was the result of that resistance which the scientific world and the medical world generally offer to new discoveries which clash with the received opinions of the day. It is perfectly true that many of the most valuable discoveries which have enriched the sphere of human knowledge and operated for the benefit of mankind have made their way against the opposition and the resistance of those who were in possession, as it was supposed, of all the scientific knowledge of the day. That is perfectly true. I suppose it arises from the composition of human nature. We do not like to have to go to school again. Those who are in possession of the field, who are the teachers of the day, who are the professors of any particular science, do not like to be told, "Here is a new discovery which will make all your boasted learning vain, which will compel you to unlearn that which you hitherto supposed to be the undoubted truths of the particular department of science to which you belong." It is all very unpleasant; and, naturally enough, if certain opinions are received into the medical world among the professors and the practitioners of that great science, and some one comes forward and says, "All your theory is without any substantial foundation, I will tell you something to show you that you have been pursuing the wrong track," there is a natural inclination in the mind of every man so circumstanced to receive the pretended discovery with distrust, and to look upon the man who puts it forward as a shallow pretender and impostor. Therefore, if a jury saw that there was any intention to crush a man so circumstanced, to drive him out of the profession because he propounded theories and proposed treatment which were at variance with the received opinions of practice of the day, I should say it was the duty of a jury to stand forth and protect a man under such circumstances.

Then why did his Lordship permit the defendant to enter into all this theoretical discussion? If a man is not to be denied justice because of his theoretical opinions, why did the Court permit two whole days to be occupied in an investigation which had no direct bearing on the case, and which could only have been entered upon for the purpose of still further injuring the plaintiff? His Lordship's words, if they mean anything, mean that the Court could only take cognisance of *acts*, and had nothing to do with *opinions* and *theories*, and yet for two days the Court of Queen's Bench *was* occupied in investigating *my* theories and *my* opinions,

and in putting a false interpretation on my words. If the object had been to murder my reputation, even at the sacrifice of truth, justice, and honour, I can imagine no course so likely to attain that end as that which was adopted.

But, on the other hand, we must equally be upon our guard against the other possible contingency. We do know, by experience which dates back to the very origin of history, that there have been men who in all ages have practised upon the fears and credulity of mankind, and especially that there have been such pretenders and impostors, as regards the ailments, the diseases, and the afflictions to which human nature is subject; men who have trafficked in the misery and the sufferings of their fellow-creatures, for the purpose of advancing their own sordid interests, without regard to how far they might aggravate the misery of those whom they pretended to cure. To denounce and expose such pretenders, to denounce those who come forward thus to practise on their fellow-creatures, is perhaps one of the most meritorious actions, or one of the most meritorious instances in which a public writer can exercise and use the ability, the talents, and the knowledge which he possesses. Therefore we must look on both sides of this question. It may be that the plaintiff while he makes dupes is himself a dupe. Many men have been the dupes of their own fancies, their own notions, their own enthusiasm. He may have thought that he has hit upon a great discovery, whereby a disease the most fatal that has ever yet depopulated society may be rendered comparatively innocuous. He may have persuaded himself that he has hit upon its true nature, and therefore that, understanding its nature, he has hit upon the remedy whereby it may be stayed. That is possible. On the other hand, and more especially if he has that medical education which his counsel boasts him to possess, he may know that the best way of working upon mankind so as to bring practice to the door of the physician, is to work upon the fears of timid and nervous persons. He may know that his pretended theory is a mere empty, vain delusion. He may know that his pretended remedy is just as delusive, and just as likely to terminate only in disappointment and death, as the ordinary treatment which he denounces. It is for you to judge between these two alternatives.

When his Lordship says, "It may be that the plaintiff while he makes dupes is himself a dupe," I cannot help inquiring, why he *assumes* what was not proved? Who did the plaintiff dupe? Two thousand patients have availed themselves of my advice since I have been in England, and yet not one out of the 2,000 appeared, to prove that he or she had been duped or deceived. If not one such could be found, why talk about

“*dupes*” and duped, as though either had been proved to exist? I do not know whether this be law or not, but I do know that it is monstrous justice.

The same observations apply to the imputation of “*working upon the fears of timid and nervous persons.*” Where were the timid and nervous persons whose fears had been worked upon?

Apart from the book, you are entitled to look to the circumstances of its publication, and, above all, to the method of advertising which has been adopted by the plaintiff. The medical men who have been called tell us that this work, even supposing that the plaintiff’s system were well founded, is altogether deficient in one of those things which are characteristic of genuine, honest, scientific professional works. If a man comes forward with a discovery of either some new theory as to a particular disease which he thinks it would be desirable for the profession to know, or if he comes forward with some new method of treatment by which he believes a disease may be cured, what is the course which is invariably adopted? He does not confine himself to a mere vague reference to the remedial agent which he thinks may be beneficially applied, he does not state the cases which have come under his experience and to which this particular mode of treatment has been applied in vague and indefinite terms, but he conveys, both with regard to the treatment to be adopted and the medicine to be given, the fullest information of which he is possessed, so that the rest of the profession may be not only enabled to judge of the reality of his discovery, but may also be enabled to apply the remedial means to which he invites them to resort in a manner calculated to secure his success. That is what every medical writer of any pretensions to the character of a writer would always and invariably do.

This is true of treatises written *specially* for the profession, but not of those intended for the *public*. A *popular* treatise on this subject should not aim at more than to enable invalids to know their diseases from the symptoms. To put before them the means of *self*-treatment would only expose them to the perils of attempting to employ remedies without the aid of medical advice and direction. To convey to them plain descriptions of the symptoms of disease—to point out to them the causes which induce, and the principles of treatment which are believed to be most judicious for the cure of, particular maladies, are all that any prudent physician would attempt in a medical work mainly intended for the public.

In like manner, the cases to which he refers are stated with the fullest detail, in order that the medical world, to whom he addresses himself, may be able to judge of how far the inferences which he draws from those cases are properly drawn. Now here the medical men, the scientific men who are called, all say that in that respect this work is altogether deficient. They say that when he speaks of sedatives, that term might convey sufficient information to the mind of a medical man; but when he speaks of inhalant alteratives, they are utterly at a loss to know what that means. But more especially do they say this—It is true you may use inhalation for the purpose of allaying irritation, as you would apply sedatives, and produce a result beneficial to health; but the great characteristic feature of your system and of your discovery is the introduction of oxygen by artificial means into the lungs, so as to supply the want of that amount of it which would otherwise be passed into the lungs through the respiratory organs, if those organs were in a healthy and normal condition. The great point you profess to have in view is to show how consumption should be cured. You do not say that consumption may be cured or tubercle be removed by the application of sedatives; it is by the application of oxygen. That is the great and important feature of your work. We all know that sedatives may be applied in the shape of inhalants; what we want to know is how oxygen is to be introduced to cure tubercle of the lung, if tubercle is to be cured by that means. You do not tell us how that is to be done, and that omission is the most important omission in your book, because the giving or withholding information of that nature in its fullest extent constitutes the characteristic difference between the honest scientific writer and the quack. The man who wants to impose on mankind keeps his remedy a secret. The man who desires that it shall benefit, not only those who are brought into contact with himself as patients, but mankind generally, by its being placed at the disposal of every medical practitioner throughout the world, gives the fullest particulars as to all those matters which it is essential should be known. That is wanting in the present instance.

With reference to any claims my professional brethren might have had upon me for fuller and more specific details, they were forfeited by their own acts before I came to England. So long as the English medical bodies persist in excluding colonial medical men from those privileges in England which the colonial profession have always freely extended to them in the colonies, there can be no fellowship, or fraternity, or claims of any kind. When a *Canadian* medical man comes to England, he does so at the peril of his liberty and his reputation. He is regarded as an alien and an enemy. He is excluded from registration; his

diplomas are pronounced worthless in the medical journals, and their slanders are copied into the public newspapers, and circulated broadcast among the people. The medical journals are closed against him, and every few weeks devote a leader to the congenial work of misrepresenting colonial and foreign medical degrees. The great aim seems to be to educate the people in hatred and ill-will against *American*, *Canadian*, and *German* physicians. Let them extend to their colonial and foreign brethren the hand of *fellowship*, and then talk to us about their *etiquette*. Until this is done, I for one spurn with contempt all claims which are based on fraternity. In no other country of the world could six medical men have been found capable of uniting to uphold a gross libel, perpetrated by a non-medical journal upon a professional brother. They will squabble among themselves, they will oppose each other's theories as experts in medico-legal trials, they will criticise violations of *etiquette*; but to band themselves together to tarnish the name and reputation of one of their own order, is conduct of which the medical profession of no other country have as yet been guilty.

They have shown us no fair play. Their assaults upon our diplomas are as false as they are malicious. They seek to exclude us from practice, to degrade us in the eyes of the public, and then they set up a claim on the ground of *etiquette*. They have for years persistently advertised their own superiority in the newspapers, and spoken of foreign and colonial diplomas as purchaseable for a few pounds. If this is a sample of *etiquette*, it is a kind of *etiquette* which I at least repudiate.

But had I been ever so much disposed to recognise their assumed claims, I could not, with prudence, have said more than I have said to indicate the treatment pursued by me in pulmonary cases, in a work chiefly designed for non-professional readers. When I say that *oxygen* is the remedy on which I chiefly rely, I say enough to indicate the remedy. How to generate oxygen, they know as well as I do; there are twenty different processes by which it can be obtained. Nothing is more easy than the generation of oxygen gas. Besides, if Dr. Williams used it twenty years ago, or ten years ago, how does

it happen that no mention of the fact is made in his books of the process by which he generated it, or of its effects in the cases on which he is reputed to have experimented? If it proved beneficial, we ought to be able to discover something about it, and there is not one word. How was I to know that he had tried it, if *he* kept the fact of his experiments a secret? Then why did he use it at all, if he did not believe the blood required more oxygen? He must have been strongly of the opinion that a little less carbon and a little more oxygen was required, or he would not have been led to make the experiment; and the same is true of Dr. Cotton, who, not convinced by the reputed but unrecorded experiments of Dr. Williams, has been aiding Dr. Biegel in similar experiments within the past *six months*. What was he trying to accomplish, if not to increase the oxygen of the blood? He could not have believed the doctrine of the chemist who thought there was too much oxygen already.

Regarding alterative inhalants, they are like alterative mixtures. Whatever *alters* the action of the part to which they are applied, or the condition of the blood, would be an alterative. This they all knew. Any dictionary of medical terms, any elementary work on *Materia Medica* would have explained it. The whole book would scarcely afford sufficient space to evolve minute details on all these points.

His Lordship thinks I have spoken harshly of my professional brethren. I can only regret that he failed to particularise my words, as it would have afforded me real pleasure to have found for him many passages much stronger in the works of the most eminent members of the profession. What I have said regarding the ordinary method of treatment was not said of individuals. No physician's feelings could have been wounded or his reputation tarnished by a general criticism of the cod-liver-oil practice. Nor could his interest have been affected in any appreciable degree. I criticised the general treatment, as I might have done blood-letting, mercury, and a host of orthodox and fashionable remedies of twenty years ago, and with less severity. I opposed their *doctrines*—they assailed me *personally*. This is a very important distinction,

They singled me out by name, and have combined together for my *individual* destruction. Their acts have been aimed at the reputation of every Colonial and American medical man who might come to England. As a sample of the recklessness with which slanderous and malicious accusations have been made, I need only instance one example. About a year ago, one of the medical journals spoke of "diplomas purchased in Nova Scotia and Otaheite." Now, it so happens that no medical degrees are even *conferred* in Nova Scotia—the great majority of the medical men of that province having actually come to England for their education, while those who did not were educated in the United States. Then, to compare a British province to an island peopled by savages may be in keeping with the spirit of the profession of this country; but if so, what particular "honour" there is in *inventing* falsehood to insult and degrade the people of a colony that has always extended to them a generous welcome, I confess myself at a loss to perceive. Anything I have said or done to justify their hostility to myself individually has been in exposure of the falsity of their representations in these respects.

But then it is said, "Oh, yes; but this was not a work intended to "enlighten and instruct the medical profession; it was to popularise "knowledge on the subject of consumption." Now, I would be the last man for a single moment to deprecate the idea of popularising knowledge upon such important matters, so far as they can be brought within the sphere of popular knowledge. I quite agree that if you can really make people sensible of what are the causes which lead to such a disease as consumption—if you can make them sensible of what are the symptoms against which they ought to be upon their guard—if you can make them understand what they ought to do with a view to prevent consumption, or what they ought to do when consumption has once been established, you are doing a vast amount of good to the mass of the community. In every branch of science, so far as you can make science matter of popular knowledge, so much the better. I rejoice when it is done in the profession to which I have the honour to belong. And we have seen in our days one of the most learned, most instructive writers, possessing erudition in the highest degree, who has published treatises which are the admiration of the scientific scholar, has popularised a vast amount of legal knowledge, and has brought it within the sphere of every man's understanding. The public ought to be eminently obliged to him for what he has done. But it is a very different thing when you come to write a treatise of

this kind, in which you denounce the whole profession as ignorant of a disease, and put forward a theory by which the fatal disease can be cured; where you hold them up to contempt and obloquy for masking the disease, and doing nothing to cure it, and say that you are the only person who can cure it, and you invite persons to come and be cured; but where you do not put the rest of the members of the profession on the same footing as yourself, and enable them in their several spheres of action to carry out the beneficial purposes which you profess to have at heart, and so do the same amount of good amongst those who are their patients that you may be able to do amongst those who are yours. That I take to be the distinctive characteristic of a man who acts honestly, for the purpose of giving knowledge to those to whom it may be important that knowledge should come. Whether they are the medical practitioners or the patients who happen to be benefited by it, it is to every one, and not only those whom he invites to become his patients, that an honest and well-intentioned writer on a subject of this kind will take care that the full amount of his knowledge is imparted.

But that is not all; I have not quite done with this work yet. Let us see the circumstances under which it is published, and see whether those are also characteristic of imposture, or whether they are consistent with the honest intention to which the plaintiff lays claim. This book goes through, as I said, edition after edition with unexampled rapidity. I can hardly see why it would be necessary to advertise it again in the *Times* to the public in the shape of an advertisement of the whole of its contents. Every man, woman, and child must have been occupied in reading this book, if we judge from the rapidity with which these editions follow one another. Yet, do you believe that these editions, which are said to have come out in the course of a month or two, and to have reached the fifth and sixth of their number, were really editions of the work in the ordinary sense in which that term is taken?

In my evidence, I stated upon oath that *six* editions of a *thousand* copies each had been published and exhausted. *The defendant subpoenaed my publisher*, and he was present in court to answer any questions which might have been put to him on this point. The defendant did *not* call him. I am unable to understand on what principle of justice his Lordship questioned the truth of my statement, when the defendant's counsel had not even attempted to disprove it. Nor do I quite see how six thousand copies of a book could put one copy into the hands of "*every man, woman, and child*" in the kingdom—the population of England and Wales exceeding twenty millions of souls.

Then the publication of extracts in the *Times* began with the issue of the *first edition*, and the "necessity" which his Lordship was unable to see, *preceded* the publication of the extracts. It was that publication which created the unprecedented sale of the book. Would it not have been as well if the *Judge* had accepted as true a statement which was *proved* by the plaintiff and undisputed by the defendant?

Besides that, it has prefaces prefixed to it, and one preface is written by Dr. MacGregor, and another by Dr. Melville. The outside world suppose that Dr. MacGregor is an independent physician, and that Dr. Melville is also an independent physician, who had both become so sensitively alive to the extraordinary merits of this all-important discovery that they cannot allow the work to go out into the world without giving it the stamp of their approval in the shape of a preface. Would it not have been as well to tell the world that these were both gentlemen acting as assistants to Dr. Hunter? There would have been more honesty, I cannot help thinking, if that course had been pursued; but, besides that, I think it is not an altogether unimportant circumstance that at the end of the work there is appended a card which announces that "Dr. Robert Hunter, author of the foregoing letters, will, during his sojourn in England, give his personal attention and supervision to the treatment of all forms of bronchial and pulmonary disease. Hours from ten to four o'clock." There may be no harm in it, but I hold that any member of the medical profession in this country, if he published a treatise upon any branch or department of that science to which he belongs, would not resort to the quack-like expedient of appending such a card as that to his publication.

This, which he censures me for not doing, is precisely the thing I did do. In the card, which he stigmatises as unprofessional, I stated that Dr. MacGregor, who wrote the preface to the first edition, *was* then associated with me in practice. Does he mean to say that I should have stated the exact terms of our arrangement? If so, I venture to submit that that at least was a matter personal to myself; concerning which neither the public nor his Lordship had any right to inquire.

So much for the book. Now a word upon the advertisements. We find that the plaintiff, not satisfied with these rapid and successive editions of the book, thought proper to have recourse to the unprecedented expedient of having the book published by instalments in the columns of the *Times*, and not only in the *Times*, but in all the leading

journals of the day. Well, I suppose if he had adopted the expedient of employing a hundred criers to go all over London in all directions and cry the work, to announce it and its merits to the public; if he had had large advertisements stuck up at every corner of each street, he could not have done anything which would have attracted public observation so much as by inserting these advertisements; because, when you take up your *Times* in the morning and see one advertisement extending from the top to the bottom of a column, and when you take up another paper, and see that in the same way filled up column by column with the advertisements, your attention is naturally arrested, and you exclaim, "Bless my soul, what is all this about?" and consequently everybody reads them.

When the first edition of my book was printed, a copy was submitted to the *Times* by my publisher, with an application for a column for the purpose of making extracts from it. The publishers of that journal referred the work to some competent authority, probably a medical man. After the lapse of several days, that competent authority reported favourably, and the column appeared; consisting of a publisher's ordinary advertisement, at the head of the column, with an extract from the work to show its character. So published, the matter was no more than a truthful description of the symptoms, forms, stages, causes, and treatment of a prevalent and fatal class of maladies. There was no more moral wrong in the publication of those extracts than there would have been in the publication of so many chapters of Paley's *Horæ Paulinæ*, or his *Moral Philosophy*. At best, it was a matter of taste or etiquette. I know of no law, human or divine, that requires me to suppress the publication of the truth, merely because its suppression would please the whims and suit the interests of others. For example, we see the speeches of barristers reported at length every day in the columns of the daily journals. They are not *less logical*, or *less eloquent*, because they appear in the newspaper press. Many a man attains celebrity and reputation in the world by having his wit and wisdom duly chronicled in the *Times*. Even his Lordship's grave and ponderous charge in *Hunter v. Sharpe*, having been reported and circulated by the newspaper press over Europe and America, may add to *his* renown as a judge, and serve to instruct the benighted people of Canada and the

United States how easily all those questions of doctrine, which have for centuries divided the profoundest pathologists into many different schools, can be settled by the Lord Chief Justice of England and a jury, especially when only the reputation of Canadian and American Doctors of Medicine is concerned.

Now the learned counsel for the plaintiff candidly acknowledges that he cannot altogether justify, or in any way approve, this course of proceeding; but, says he, this gentleman comes from America, and in America they do strange things in the way of advertising. It is thought nothing of there; you may advertise to any extent; it is only a legitimate and ordinary mode of pushing yourself into notoriety. Gentlemen, we are not in America; we are in England; and whatever the practice may be in America, I am happy to think that such a practice has not hitherto obtained in this country. Empirics advertise; professional men do not; and no one, I think, can doubt—and I am sure that that highly-gifted and distinguished member of the bar would be the last man to think what I am about to say is not true—that if it were open to professional men thus to advertise their theories and modes of treatment in the *Times* or any of the other great journals of the day, the dignity, the honour, and the respectability of the profession, members of which were allowed to do so, would be tarnished and sullied to a degree perfectly lamentable. A man who invents a machine, or is the author of any of those discoveries which are connected with the commerce of the day, has no other means of making them known than by advertising; no one blames him for doing it. But members of a profession do not resort to that means of making themselves known to the public, and endeavouring to acquire professional position and professional advancement. What would be thought of a member of the bar who, having published one of those treatises by which we are all from time to time enlightened and instructed in our professional knowledge, were to take portions of it and from day to day advertise them in the columns of the newspaper, taking care to append this important piece of information, that “Mr. So-and-so, the author of this very learned and valuable treatise, sits in his chambers from four to six, and will be happy to advise all those who may come to him upon the subject of the treatise?” Why such an individual so acting would be scouted from the profession which he would be thought to have humiliated and disgraced by such conduct. What difference is there between the two professions? They are sister professions, equally guided by the same rules of professional honour and professional propriety; and therefore I must say, whatever may be said of America, that I do not believe, and will not believe, until I have it on some evidence of which there is at present none, that such proceedings are resorted to on the part of members of the medical profession or of any other profession in America. All I

can say is, that it is not consistent with the notions of professional propriety and honour to do so here, and we must not forget that the writer of this article, who is now sought to be made responsible for its contents, was writing with reference to that which is done in England, and which he thought, as a member of the profession, was inconsistent with professional propriety.

His Lordship is slightly in error. All *English* medical men who write works do advertise, and in the daily papers too. If he will take up the advertising sheet of the *Times*, almost any day in the week, he will see often two or three columns of such advertisements, and of books written by leading members of the profession. The prices, too, are such as scarcely to suggest the idea of *commercial* enterprise—viz., from “*ten stamps*” up to four or five shillings a copy. It is true that these are mostly epitomes of larger works, and that they are put forth *for the public*, not the profession. I wonder how many of them embody ample details of the methods of treatment recommended, such as would enable me to practise what they profess to teach? Lest I should be again accused of misrepresenting my professional brethren of England, I will append a few of *their* advertisements, that they may be compared with my publisher’s advertisements; for, *of course, it is all done by the publishers*; only the physicians *pay* for having it done.

Svo., cloth, 7s. 6d.

DISEASES of the HEART and GREAT VESSELS; their Pathology, Physical Diagnosis, Symptoms, and Treatment. By H. W. FULLER, M.D. Cantab., F.R.C.P. Lond., Physician to St. George’s Hospital. “Thoroughly practical. The remarks on the prognosis of the various forms of disease of the heart, on its dilatation, fatty degeneration, and functional derangement, and on the means of distinguishing symptoms of functional derangement from those of organic disease, will be appreciated by those who have felt the difficulties of this class of diseases.”—*Medical Times and Gazette*. John Churchill and Sons, New Burlington-street.

Just Published, Sixth Edition, price 2s. 6d.; per post, 2s. 8d.,

DR. HUNTER on the LUNGS.—Practical Letters on the Nature, Causes, and Treatment of Catarrh, Sore Throat, Bronchitis, Consumption, and Asthma. By ROBERT HUNTER, M.D., formerly Editor of the “*New York Journal of Diseases of the Chest*.” With a Preface to the first English Edition, by J. J. Macgregor, M.D., M.R.C.S.E.; and Introductory Observations on the Efficacy of Medicated Inhalations, illustrated by English Cases to the Fifth Edition, by Henry Melville, M.D.

London: C. Mitchell and Co., Red Lion-court, Fleet-street; and may be had of all booksellers by order.

Fep., cloth, price 1s. ; free by post, 1s. 2d.,

LAW (The) **RELATIVE** to the **SALE** and **PURCHASE** of **HORSES** ; comprising Notices of the Chief Statutes and Decisions thereon, collated to the present time, and the Leading Cases on Warranties ; with a copious Index, and Table of the Authorities cited, and Observations on Letting, Hiring, and Driving Horses, &c. By **CHARLES EGAN**, Esq., Barrister-at-Law. London : William Tegg, Pancras-lane, Cheapside.

MR. JEAFFRESON'S NEW WORK.—Now ready, 2 vols. 8vo, 30s.,

A BOOK about **LAWYERS**. By **J. C. JEAFFRESON**, Barrister-at-Law. "These volumes will afford pleasure and instruction to all who read them, and they will increase the reputation which Mr. Jeaffreson has already earned by his large industry and by his great ability. We are indebted to him for about 800 pages, and that rare and valuable addition, a capital index, all devoted to the history and illustration of legal men and things. It is much that we can say for a book that there is not a superfluous page in it."—*Athenæum*. Hurst and Blackett, publishers, 13, Great Marlborough-street.

Price 14s.,

CANCER of the **BREAST**, with Four Coloured Engravings. By **VELPEAU**. Translated from the French by **WILLIAM MARSDEN**, M.D., Founder and Principal Surgeon of the Cancer Hospital, London and Brompton. "A literal and most faithful translation of a most valuable treatise. It is not enough to say that it merits the confidence of all. The work is destined to become popular, and to be a standard of reference for a century at least."—*Lancet*. London : Henry Renshaw, 356, Strand.

Second Edition, post 8vo, cloth, 8s.,

ON the **PHYSICAL DIAGNOSIS** of the **DISEASES** of the **LUNGS** and **HEART**. By **HERBERT DAVIES**, M.D., Cantab., Consulting Physician to the Royal Infirmary for Diseases of the Chest. Physician to the London Hospital. "The work of one who is evidently an excellent auscultator."—*Med. Chir. Quarterly Review*. John Churchill and Sons, New Burlington-street.

13th Thousand, price 2s. 6d., post free 32 stamps.

HUNT on the **SKIN** : a Guide to the Treatment of Diseases of the Skin, Hair, and Nails ; with cases. By **T. HUNT**, F.R.C.S., Surgeon to the Dispensary for Diseases of the Skin, 17, Duke-street, Manchester-square. "Mr. Hunt has transferred these diseases from the incurable class to the curable."—*Lancet*. London : T. Richards, 37, Great Queen-street, W.C.

Even his Lordship would find it difficult to point out any substantial difference between these advertisements. I am unable to discover from them why one should be regarded as right and proper and another not so. I presume they are all intended to benefit in some way their authors. I know of no reason why they should not do so. No man has a right to comment injuriously upon my acts when he does precisely the same thing himself. Every medical man in England who writes a book advertises it in the newspapers, and hopes to make it the stepping-stone to fame in his profession.

I say, therefore, gentlemen, that you are at liberty to take that into consideration, as well as the other remarkable things which are to be found in this book and in the conduct of the plaintiff; and the question for you upon all this will be, whether you are of opinion that substantially the plea of justification is made out, that the plaintiff did seek to work unduly for sinister purpose on the fears of those into whose hands his work should fall; that he sought to exaggerate the symptoms of these various complaints with the view, when he had excited the apprehensions of readers to a sufficient extent, to make them believe that there was only one remedy for these diseased organs, and that his was that remedy, and he was the only person capable of applying it. It is for you, I say, gentlemen, to say whether, in putting forward these things, you consider he did so honestly, with a belief in the truth of what he stated, or with a sinister design. Upon that you must form your own judgment.

This I have shown, by parallel extracts from the most eminent members of the profession, I did not do in any single instance, therefore that falls to the ground; and every observation which was based upon it, only adds to the outrage inflicted upon me.

Now, in addition to the defence of his theory which he and his associates (for they call no one else) put forward through their evidence, he calls your attention to certain cases in which he has effected a cure. Now I was looking at these cases, and, upon making a short analysis of them, I found this to be the case: twelve witnesses were called—of these seven were cases of asthma, two were cases of chronic catarrh, three were cases of bronchitis. Of the latter, one had been told that he had tubercular deposit; another had been told by another practitioner that one lung was affected, and the other slightly affected. Therefore, out of these twelve cases, there were but two in which it could be suggested that there was any tubercular disease.

Now it is admitted—and I think that this part of the case is certainly deserving of your attention—that as regards the cognate diseases which, according to the plaintiff, lead up to consumption, inhalation is of considerable use, especially the inhalation of sedatives, and that it has been somewhat largely resorted to by medical practitioners for a long space of time. The point upon which the question, as it seems to me, turns with regard to the honesty or dishonesty of the plaintiff is, whether he can have believed that the administration of oxygen would, in the way he has described, remove the mischief arising from tubercular deposit in the lungs. Is his case advanced by the instances of cure he has undoubtedly brought forward? Tubercular consumption is that which he proposes to grapple with. He does not say that the other diseases are fatal, except so far as they lead up to it; “but when they do,” he says, “they are, under the present “system, incurable, and I have cured seven cases of asthma and two

"cases of chronic catarrh," neither of them being consumption. What about the other three cases? In one of these he told the patient that he had tubercular deposit: there was no other evidence of it. In another case it is true that another medical man told him that his lung was affected. Whether that meant that tubercular deposit was there, is a question that remains open. Then there is the instance of the gentleman from Canada. I allude to one of the witnesses who spoke in a deposition, and who says that he certainly had laboured for some years under some affection of the chest, and who complained of being in a consumption; whether he was so or not we do not know. So with regard to the Polish Countess. No doubt she had an affection of the chest, but it appears that her own medical man was of opinion that that was not tubercular disease, that it was not consumption. Says the plaintiff in his report of the case, "We thought differently." They sent her out the usual remedies, which were administered, and in the course of two months she was perfectly well, and never had recourse to a medical man. Are you satisfied that that was a case of tubercular consumption?

I think his Lordship's analysis scarcely does justice to the cases. There were *three distinct, well-marked cases of consumption*. Two of them had been *so pronounced by the medical men previously in attendance upon them*. Then there were *two cases of Bronchitis, closely simulating consumption*; two cases of *inveterate scrofulous catarrh*, and *four cases of confirmed asthma*. These patients, with one exception, had been under the care of leading medical men, without deriving any permanent benefit; several of them had even been made worse. They were, as a test of the efficacy of the practice, as valuable as a *hundred cases would have been*. Besides, it was a work of supererogation on my part to put in a single case. I did not go into court to prove the *efficacy* of my practice. That was not involved in the issue. I had been accused of "inveigling patients into my consulting-room," and of "extorting enormous fees." Of practising a system of medicine which did no good, but injury. Of putting myself forward as an English M.D. Of being a "scoundrel," and of "CLAIMING TO BE RESPECTED ON THE FAITH OF DEGREES OBTAINED IN CANADA AND THE UNITED STATES." It was not necessary for me to have submitted a witness. I appealed to the Court to compel the defendant to prove the allegations which he had made against

me, and he failed to produce a single witness to uphold them. Nobody had been “inveigled”—“suffered extortion of money,” or injury of health. There was not a soul to say that he or she had been treated with any want of kindness, consideration, or skill. Lastly, there was the crime of claiming to be respected on the faith of my Canadian and American diplomas! That I was prepared to admit; and I was further prepared to show that I could have obtained an English diploma on a much lower course of study. Why was that not gone into? It was surely a rare opportunity for the profession to have justified its libel upon the Colonial and American Colleges. The action was, to all intents and purposes, an *undefended* one; and every particle of evidence put in on behalf of the defence was irrelevant and foreign to the issue.

All this gives rise to the question as to whether imposition has not been practised upon the patients, and as to whether they were not led to believe that their lungs were affected, when in fact it was simply one of these forms of bronchial irritation which are common, but in which the bronchial tubes, though not part of the lungs themselves, yet being immediately connected with the lungs, and ramifying all over them, being affected, it sometimes happens that bronchitis is mistaken for consumption, or at all events is spoken of under that denomination. I have adverted to this, because cases were very much relied upon by the plaintiff as showing that whatever might be the theory opposed to his own, whatever might be said theoretically on the part of physicians with regard to his treatment, yet the practical results of it went to establish that his theory, supported by these practical instances of success, was superior to that of his opponents, who could only negative theoretically that which he advanced.

Does his Lordship not know that, with the prevailing idea regarding consumption—viz., that it implies a taint in the family blood—not one person in a thousand has the moral courage to come forward and say, I had consumption and Dr. Hunter cured me, and that, too, when there was no *necessity* whatever for doing so? It would have been easy for me to have produced hundreds of recoveries, could I have done so without exposing the names of my patients. This my counsel distinctly stated, but the Chief Justice passed it over without a word of comment, and even reflected on me because I had not brought

forward more cases of cure. The defendant, in his subpoena, required me to produce the original letters of every case reported in my book, with the prescriptions employed in their treatment. These I had in Court ready to produce. How was it that I was not asked a word in regard to any of them? When Dr. Melville took the stand he was required to produce the original letters of Case VIII. The letters were immediately produced, and, after being subjected to a very close scrutiny, were pronounced by his Lordship "*unquestionably genuine.*" After this little episode no further letters were demanded by the defendant's counsel. He saw that he would only be giving me an opportunity of proving the authenticity of each case reported; and that, of course, he did not want to do. Now, consider the case of the Countess to be one of consumption or chronic laryngitis, or bronchitis. What does it matter? She had a persistent cough, expectorated yellowish and at times greenish mucus; had quick pulse, and cold hands and feet. These symptoms had, moreover, continued for about two years, viz.—from the spring of 1863. She promptly recovered under the remedies prescribed, and those remedies were *entirely administered by inhalation!* At the end of two months she was quite restored. Does this not speak well for the system of treatment?

No! not in the opinion of his Lordship. On the contrary, he says, "*it gives rise to the question whether imposition has not been practised upon the patients!*" In order to realise the peculiar force of this observation, it is necessary for the reader to understand that the patient had made no complaint, and that there was no evidence before the Court to show that she had any knowledge even of what my opinion was. But even admit she had been told that she was in consumption—what then? Could his Lordship pretend to know that she was not? Must my diagnosis agree with his? Would I even be open to his imputation if my *diagnosis* was entirely erroneous? If so, I fear every medical man in London is in danger, for if errors of diagnosis be proof of imposture, then not ten need hope to escape, and those only because they never had a patient! I question the *right* of any Judge to raise a question of this

nature on a matter concerning which there was neither *imputation* nor *evidence* before the Court.

Now you, gentlemen, must say upon the whole whether you think, taking all the evidence into account with the various parts of the book, that the plea of justification is made out or not. But now for one moment suppose it to be your opinion that you could not go the length of saying that imposture in the proper sense of that term was established as against the plaintiff. Imposture as used in this article, and I take it as used in the ordinary parlance of mankind, means intentional deception practised on the rest of mankind. If a man deceives himself and deceiving himself deceives you, he deceives you no doubt; but it cannot be said that you would be justified in saying that he was an impostor. It may be, but it is entirely for you to form your conclusion, that although the plaintiff's theories are delusive, although his treatment would not be attended with the beneficial results which he holds out to the world, nevertheless he may have honestly believed in that which he professed to write and in the practice which he purposed to administer; but that does not conclude the question, but only brings the defendant to his second ground of defence, which in that case it would be necessary for you to consider.

Under his second head of defence he says—This was a matter of public concern. The plaintiff put forward in these advertisements his system, his theory, and communicated to a certain extent the remedies by which he proposed to cure this malady. He invited persons to come to him for the purpose of being treated and cured by him. I could see, from his account of the theory on which his treatment was based, and from his account of the treatment that he proposed to use, that the whole was a mere idle delusion, and looking at the mode in which his work was published, looking at the secrecy in which his discovery was shrouded, looking to the mode which he adopted to bring himself into notoriety—a mode which was utterly at variance with the received habits of the profession to which he professed to belong—I was warranted in drawing the inference that he was a mere pretender, and not only a pretender but a quack who intended to impose on mankind as quacks do. I was warranted in drawing this inference, and I denounced him accordingly. It may be that I was wrong, it may be that now the matter has been fully investigated, and that the plaintiff has had an opportunity of being heard and vindicating this theory which he has put forward, of showing that his practice has not been wholly unsuccessful, that a jury may think I have gone too far, but the question is not thereby concluded, if it should appear, under all the circumstances of the case, that, bringing to the discharge of my duty as a public writer caution and moderation in criticising what I thought to be a mischievous and noxious pretence, I have exercised a reasonable and careful judgment, have not been over hasty and precipitate, inferring sinister design and wicked motive against the

person whom I have assailed. If I have brought to the discharge of my duty only an honest desire to do good and benefit in the department to which I belong, in that case, if a jury should be of that opinion, I am entitled to their verdict. And, gentlemen, I endorse that proposition. Here is a man challenging public criticism by bringing forward what professes to be a scientific book, inviting the public to come to him to be treated for the saddest disease that is known amongst us. If he does that, he challenges public criticism; and then, if a public writer of competent knowledge deals with his theory, and, looking upon all the circumstances, using that forbearance and moderation and exercising that temperate judgment which every man is bound to exercise who not only criticises the conduct of another, but proceeds to impute to him evil motives and designs—if the public writer executes his task with that spirit, goes beyond the limits to which a more sound knowledge of the facts would have warranted him in going, he is nevertheless privileged; the occasion is a privileged one, and if the privilege is exercised honestly and faithfully, and with reasonable regard to what truth and justice require, he is exempt from the consequences if he shall have gone beyond what the limits of truth more carefully ascertained would have justified. It is, therefore, not necessary that justification should to all intents and purposes be made out, if you think the defendant or the party who wrote this article for which the defendant is made liable was, in the reasonable and honest exercise of his duty as a public writer, warranted by the circumstances in drawing the inferences which he has drawn as to the motives and conduct of the plaintiff, although it may turn out that he has not been to the fullest extent accurate. Upon what you may think upon one or other of these grounds of defence will turn your verdict.

Let me assume that you are in favour of the plaintiff. You will have to consider all the circumstances. Undoubtedly, if the attack was unwarranted, it was most painful, and one likely to be very disastrous in its consequences, and I cannot help thinking it was made at an inopportune moment. All those allusions to Mrs. Meyrick's case, at all events, should have been sunk for the time; but, as I have said before, that does not appear to have done the plaintiff very much harm. At the same time it is unquestionably an article conceived and written in a spirit of extreme bitterness and extreme severity; the language is of the strongest description, and there can be no doubt that it must have inflicted great pain on the man who was the object of an attack of that description; and that, unless conduct so assailed and impugned can be vindicated by the verdict of a jury, it must place him in a very disastrous position as regards his future life; but, on the other hand, you must not, if you think your verdict ought to be for the plaintiff, forget the circumstances under which this article was written.

The learned counsel suggested for the plaintiff that it must have been some medical man who wrote it. I think that is extremely

likely, and I hope it is so, because in that circumstance I find that which would induce me to look with indulgence, and perhaps would induce you to look with indulgence, upon the article, even if you think it cannot be justified in itself; because it is plain to a man who reads it, that that article was written in a spirit of honest indignation; that that indignation was such as a member of the medical profession might well feel at seeing the way in which a member of the common profession, although not a member of the English part of it, was lowering, degrading, and soiling the profession of which he was a member. I think, therefore, that if it be the fact, as suggested, that it was some medical gentleman who wrote that article, great allowance is to be made for it. Here was a system of advertising resorted to, which of course was humiliating to the profession of which we will suppose the writer to be a member. Moreover, he believed (he may have been right or he may have been wrong) that the system put forward by the plaintiff, and the treatment to which he professed to resort, was altogether illusory—perfect moonshine—and only put forward for the purpose of deception. Moreover, there had happened a thing which had startled the world. This gentleman had not been in practice for more than a very short time, when a charge was made against him by a female patient of having violated the honour of his profession by seeking to abuse the opportunity of her resorting to him as a patient to further his own purposes, in a way to which I need not further allude.

Vile accusations of the nature to which his Lordship alludes are not so rare in England that this occurrence need have *agitated* society to its foundations. Similar charges have certainly been made half-a-dozen times each year, and have even threatened the honour of several of the proudest names in England. It is one of the common artifices adopted by a certain class of desperate women to *extort money*. Why should this, which was so monstrously improbable, if not impossible, that the Jury pronounced it false without a witness being called in my defence, have “*startled the world*,” when much graver charges made against much more prominent names were treated with contempt by the whole community?

If the writer of that article was satisfied, as I think you cannot fail to believe he was in his own mind, that the advertiser—the plaintiff—was an impostor, a pretender, and a quack; and, further, was led to think, until the imputation was disproved by the verdict of a jury, that this man, whom he believed thus to be an impostor, so far as regarded his practice as a medical man, was also a man capable of

abusing the confidence which a patient placed in him as a physician to further his own purposes, one cannot wonder that a man should have sat down, and in the honest indignation of the hour put gall into the ink with which he exposed and denounced, as he thought he honestly might do, the conduct of the person who he believed had so disgraced himself and degraded his profession. You must look at all the things taken together. It is not like a man sitting down deliberately and writing that which he knows to be false, with the view of injuring another. It is not like the case of a man who, from political antagonism, or any other cause of rivalry in the breast of man, sits down and draws hastily unwarrantable inferences of bad motives from circumstances which do not warrant such inferences. It is the case of a man who honestly believes he is exposing and denouncing an abominable system of quackery and puffery, who may have been mistaken, who may be responsible, whom you may hold to be liable to a claim for damages for the purpose of vindicating the plaintiff's character; but we must not have it said of him—because I think it cannot be said justly of him—that he was actuated either by personal or professional malice, or any other than an honest desire to discharge his duty towards the profession of which he was a member, and the public in whose interest he was writing.

If the writer of the libel *had* been actuated by “honourable motives,” he certainly would have confined himself to what he knew to be true, or to a general criticism of the theory on which my practice was based. He might have launched into the question of medical etiquette to his heart's content, without the least impropriety. His conduct might have been proof of bad taste; but, if so, it would have been treated by me with silence, if not with contempt. What he did was to select a moment when I was unable to defend myself, and when my good name was doubly valuable to me, to invent and put in circulation a series of false, filthy, malignant accusations, calculated to *ruin* my professional interests, to *degrade* my family, and to *endanger* my liberty.

I entirely agree with the Chief Justice's supposition that the libel was written by a medical man. Certainly, “envy, hatred, malice, and all uncharitableness” run through it, and bear evidence of its origin. It was not, however, in spirit, materially different from the *scientific* evidence delivered before his Lordship, and it is quite possible that that fact suggested to his mind its probable source. “The writer,” says his Lordship, “put gall

“into the ink,” and then he deliberately sat down, and invented a tissue of unparalleled and unmitigated mendacity! There is nothing in his style to indicate that his mind was materially trammelled by scruples of conscience. He evidently meant to say enough to accomplish his purpose, and he said it without the slightest regard for its truth. If his professional brother’s character was to be destroyed, if he was to be robbed of his liberty, if his name was to be dishonoured, and his children reduced to beggary, now was the time to deal the fatal blow; not a moment was to be lost—only two days intervened before that investigation which might for ever blast, or again restore his name untarnished to society. It was under *these* circumstances that the blow was struck! It was a cowardly act. It was as *base* as it was *false*, *wicked*, and *cruel*.

If these be proofs of “*an honest desire to discharge his duty to the profession of which he was a member*,” then that profession must have regarded my destruction as an object to be attained by any means, *false* or *foul*, which might be found necessary; the writer of the libel in the *Pall Mall Gazette* may have been one of their instruments, and the cigar-woman in Baker-street may have been another, the one assailing my *professional* and the other my *moral* character, and both working together for the attainment of the same end.

The Chief Justice says, “*It is not like a man sitting down and deliberately writing that which he knows to be false, with the view of injuring another.*” But on what grounds does his Lordship assume that the writer of this libel did not *know* it to be *false*? It is clear that he did not know it to be *true*—nay, more, from his failure to produce a tittle of evidence in extenuation, the inference is inevitable that he had no grounds whatever for even supposing it *might* be true. The journal in which the libel appeared no doubt made money by it as a piece of sensation scandal. But let us admit that the writer was utterly ignorant of me and of my personal and professional character. Is that a sufficient justification for the publication of a damaging libel, which tended to degrade my family and destroy my means of livelihood? This justification by *ignorance* furnishes a prece-

dent which, practically, opens wide the door of escape to the foul fiend of slander.

If ignorance may be pleaded in justification of libel, then every filthy creature may pour out his venom with impunity; the *Pall Mall Gazette* may become a regalanized *Satirist* or *Billingsgate Mercury*, and a spirit of Satanic malignity may enthrone itself in the editorial seat and launch its poisoned arrows with impunity against the fame, the honour, and the interests of every family in England. This *new precedent* effectually shields the libeller from punishment, while it practically bars the door of justice against the injured.

Gentlemen, I have made all the observations which I think it necessary to make to you. The case is one of great importance. You have paid the utmost attention to it, and therefore you are in a condition to discharge your duty, and to say which way your verdict shall be given.

After two hours and forty minutes' deliberation, the jury returned a verdict for the plaintiff. Damages, one farthing.

CONCLUSION.

As this verdict is unparalleled in the annals of justice in this or any other country, I am in doubt whether it is to be attributed to the fact that the defendant failed to produce a single witness in support of his allegations—or to the admission of wholly irrelevant matter, which neither the Judge nor the Jury did or could understand—or to the fact of my being a Canadian physician, who dared to return to the land of my birth, and “*claimed to be respected in England on the faith of collegiate degrees obtained in Canada and the United States!*” I shall leave the people of England, of Canada, and of the United States to solve this difficult problem.

There is, however, one gratifying reflection in connection with this trial which cannot fail to occur to the minds of most, and that is, that we have at last discovered how all those vexed questions of medical theory and doctrine which have for ages defied the astute pathologists and grave Doctors of

Medicine throughout the world, can in England be settled by the Lord Chief Justice and a Middlesex jury! Had the same enlightened ideas prevailed in the days of Galileo, of Jenner, of Harvey, and of Newton, the world might still have been a plain resting on the back of a turtle; the Archean spirit would certainly have reigned supreme in the arterial tubes; the small-pox have served to prune and keep down our redundant population; while Newton would never have been such a fool as to notice the "fall of the apple," unless it were to follow the example of our primal ancestor, and *eat it*!

UT SEMENTEM FECERIS ITA METES.

ON THE NATURE OF CONSUMPTION.

The reader will naturally expect to know, since my theory has been pronounced *false* by Dr. Williams and the other medical witnesses for the defence, what theory is *true*. The following list of theories, held by the medical profession in different ages of the world, was prepared by Ancel to illustrate "*the successive phases of medical doctrine.*" Now, were it possible for a physician to have lived through these ages, it will be seen that he could only have preserved his *orthodoxy* by changing his medical *faith* about *fifty* successive times! And had the Chief-Justice of England and his Middlesex non-medical jury flourished during the same period of time, they might have decreed the very same physician an "impostor," a "quack," and a "scoundrel," at least *fifty* times for not *following the medical fashions!*

HIPPOCRATES, B.C. 432.—*Scrofula*.—A lymphatic temperament and an exudation of a morbidic humour. *Consumption*.—A defluxion of matter from the head corroding the lungs.

ARISTOTLE, B.C. 343.—*Consumption*.—A specific contagion. (Young, p. 121.)

GALEN, A.D. 170–180.—*Consumption* (*Phthöe*).—Follows Hippocrates. An ulceration which eats into the lungs, so that the whole of the body is consumed thereby, depending on a certain constitution produced by the influence of a morbidic cause. Tubercles formed by glutinous, thick, or certain other humours in the lungs. Distinguished the symptoms of tubercles from those of bronchitis.

ARETÆUS.—*Consumption* (*Phthöe*, *Phthisis*).—An abscess of the lungs, attended with constant cough, and with bloody or purulent expectoration.

CÆLIUS AURELIANUS.—*Consumption*.—A mixture of the *strictum* and *luxum* of the methodic sect.

AMBROSE PAREY, 1579.—*Scrofula*.—A gross cold viscid humour mixed with melancholia; the local disease bearing a proportion to the *materies morbi*.

SENNERTUS, 1627.—*Consumption*.—An acrid sanies generated in the lungs.

- SYLVIUS DE LA BOE, 1650.—*Scrofula*.—The first to intimate the connexion between scrofula and consumption. *Consumption*.—Scrofulous degeneration of insensible glands in the lungs. Believed that tubercle arises from certain glands in the lungs, resembling those of the neck and mesentery : an opinion subsequently adopted, with some modifications, by Willis, Morton, Sauvages, Webber, Portal, Broussais, and others.
- CHRISTOPHER BENNETT, 1654.—*Consumption*.—The blood contaminated by unwholesome vapours, damp inclement winds, errors of diet producing bad chyle, unsuitable labour, want of exercise. sadness of mind, &c. A low vitality of the blood (*Theatrum Tabidorum*, p. 138, &c.).
- THOMAS WILLIS, 1660.—*Consumption*.—An acidity of the blood, a general acrimony of the humours, and a wasting away of the body from a base conformation of the lungs. Condemned the definition of an ulcer of the lungs, as he found in some cases only tubercle or calcareous and sandy matter.
- WISEMAN, 1676.—*Scrofula*.—*Scrofula* and *Phthisis* identical. A peculiar acidity of the serum sanguinis, which coagulates and hardens glandule, membrane, or muscles, and dissolveth and rotteth the bone.
- RICHARD MORTON, 1680.—*Consumption, Tubercle*.—Distinguishes between atrophy and consumption. *Consumption*.—A peculiar morbid secretion, by muciparous glands, of lymph from the blood, and a depraved condition of the blood, the chief elements. *Seat of Tubercles*.—The pulmonary vesicles and ultimate bronchial ramifications. *Phthisis*.—Either congenital or acquired, chronic or acute. Thus, he regards consumption as a pulmonary tuberculosis, caused by a morbid state of the blood, which may arise from a vicious conformation of the chest, or from other direct causes ; or may depend on other diseases capable of generating an unhealthy lymph, or an alteration of the blood. He distinguishes a threefold variety of tubercles—1. Semipurulent. 2. Having a steatomatous appearance of various colours and consistence. 3. Resembling the medullary substance of the brain. Pointed out the frequent connexion between scrofula and tubercles (*Phthisicologia*).
- ETMULLER, 1680.—*Consumption*.—Said to be the first who confuted the error that any phthisis is produced by a fluxion from the head.
- SYDENHAM, 1680.—*Scrofula, Consumption*.—An acrid humour ; by means of the salivary ducts and the glands of the fauces is carried through the trachea into the lungs. A crude phlegm formed in the lungs, and a putrid miasm scattered over the body. Phthisis is scrofula of the lungs.
- BOERSHAAVE, 1709.—*Consumption, Scrofula*.—A conversion of the blood and chyle into pus. A depravation of the humours from a putrid cacóchemia.

- MARTEN, 1722.—*Consumption*.—Animalculæ in the lungs. (Quoted by Dr. T. Reid, 1785.)
- WEBER, 1727.—*Consumption*.—(Quoted by Bonnet.)—Explained for the first time the origin of the pulmonary cavern : “Pulmonary vesicles, the walls of which are like to the web of spiders, partly become thickened, partly are broken, so that from many vesicles one or several large caverns result.” “Crude tubercles precede this calamity.”
- VAN SWIETEN, 1741.—*Consumption*.—From putridity. Insists that the affection of the lungs is that most frequently connected with the various kinds of consumption described by his predecessors.
- GIRTANNER, 1743.—*Scrofula*.—Debility and irritation of the lymphatic system.
- CHARMETTON, 1752.—*Scrofula*.—Fixed or volatile salt, with acid or bitter earthy particles, which thicken the juices, especially the lymphatic fluids (*Essai Théorique et Pratique*).
- BORDEAU, 1760?—*Struma*.—A peculiar acid in the blood, producing an acid coagulation of the serum.
- MORGAGNI, 1761.—*Consumption, Scrofula, Tubercles*.—With Sylvius and Morton, says the consumptive constitution is always manifested with tubercles ; these, the result of bad nourishment from disease of the stomach. Gives cases in which internal tubercles and scrofula are conjoined. From observations by Valsalva proves the superior lobes of the lungs the principal seat of tubercles, and gives as a reason that they are less active in respiration owing to their distance from the inferior ribs and diaphragm. First made known tuberculization of the bronchial glands, admitting the possibility of their communicating in disease with the bronchial tubes. Perhaps the first to discover the miliary form of tubercle.
- DE HAEN, 1761.—*Scrofula, Consumption*.—Alteration of the fluids consequent on the small-pox. Pus in the blood in the form of a noxious humour.
- CULLEN, 1777.—*Scrofula, Consumption*.—Probably some noxious acrimony of the fluids. Expectoration of purulent matter, probably containing the same noxious acrimony that prevails in scrofula, but may sometimes be exanthematic or syphilitic.
- JOHN BROWN, 1780.—*Consumption*.—A mixture of the sthenic and asthenic diathesis. *Scrofula*.—Local debility. A want of due balance in excitability between the lymphatic and other parts of the system.
- BAUMES (Montpellier), 1783.—*Scrofula*.—Considered that he had proved the existence of an acid principle of a phosphorous or phosphoric nature, which in the first instance thickens the lymph and renders it liable to concrete, and ultimately tends to putridity. He founded this theory upon a presumed connexion between scrofula and rachitis, and scrofula and gout ; upon the

frequency of affections of the bones, with destruction of their earthy material; upon the acidity in the early stages and the acrimony in the later stages, of the secreted fluids; upon the collections found after death of phosphatic salts in the glands and viscera; and upon an observation of his own, that in the earlier stages of scrofula, and even before the local disease declares itself, the urine contains a diminished proportion of phosphates; whereas in confirmed cases, according to Fourcroy's analysis, the phosphates are increased. Baumes remarks that it is uncertain whether this "acido-putrid" depravation takes its origin from the morbid blood in its passage through the glands, or from a fluid exhaled from the blood into the general cellular system, or whether it is formed primarily within the lymphatic vessels. Hufeland entertained a similar idea of an acid principle in the lymph, and the whole theory of tuberculous diseases has frequently been founded on this assumption. *Consumption*.—A specific contagion distinct from scrofula.

DR. WHITE, 1788.—*Scrofula*.—Obstructions of the lymphatic system by a viscidly of lymph, all the humours having a tendency to become acid.

RUSH, 1793.—*Consumption*.—Not identical with scrofula. Depends on debility—a primary affection of the system, not of the lungs.

WEBER, 1793.—*Scrofula*.—Excess of oxygen.

DR. BEDDOES, 1793.—*Scrofula*.—Deficiency of oxygen. Inirritability. *Consumption*.—Hyper-oxygenation of the blood. Oxygen (the acidifying principle) combines with the purulent matter, and thereby tends to enlarge pulmonary ulcers. Florid colour of the blood, redness of the tongue and lips, and the blush on the cheeks show that the blood abounds in oxygen. High situations suitable for the consumptive, as containing less oxygen.

SÆMMERING, 1795.—*Scrofula*.—Relaxation, debility, and dilatation of the absorbent vessels.

BAILLIE, 1795.—*Tubercles*.—The essential cause of consumption. Found them in children from two to three years old. Demonstrated their seat in the cellular tissue, and their nature as different from that of glands.

SPRENGEL, 1796.—*Scrofula*.—Quotes another author, that it depends upon a peculiar virus and a viscous state of blood, with the development of an aeriform fluid in the body.

HUFELAND, 1797.—*Scrofula*.—A deep-seated atony of the lymphatic system, accompanied with a specific irritation of this same system, and a particular alteration of the lymph—an acrid acrimony of the lymph.

DR. T. SUTTON, 1799.—*Consumption*.—Obstruction of the mesenteric glands, the proximate cause of all the circumstances belonging to the disease. The fatality of this disease is not owing to the hectic fever, nor to the local affection of the lungs, but the chief

cause of the emaciation and debility is some disease of the abdominal viscera. The blood is diseased, there being "a deficiency of gluten," occasioned by the nourishing part of the food being prevented arriving in the blood-vessels. Refers to the connexion subsisting between mesenteric disease and phthisis, to the fact that many cases of phthisis are preceded by affections of the bowels, and that consumptions frequently succeed to protracted dysenteries, and other chronic complaints of the abdominal viscera. The affection of the mesenteric glands adequately accounts for the "most important symptom—the emaciation—and this symptom precedes any material disease of the lungs.

CABANIS, 1804.—*Scrofula*.—Atony of the lacteals and ganglions, and exaggeration of the lymphatic system.

BROUSSAIS, 1804, 1808.—*Consumption*.—Depends on a chronic phlogosis, inflammation of the lungs, or catarrh, parenchymatous, or bronchial, which, if prolonged by the cause that produces it, could impress the lymphatics of the lungs with a certain impulse, leading to the formation of tubercle, or various deposits of tubercular matter. He states that inflammation of the pleura, leading to compression of the parenchyma of the lungs, may determine the development of tubercles. "As every pulmonary inflammation, when prolonged, may determine lymphatic inflammation, therefore the production of tubercles should be attributed to the continued inflammatory action of bronchitis, pneumonia, pleurisy," &c. He admitted, however, that in certain very weak individuals tubercles develop themselves without the symptoms of inflammation; but his theory is, that tubercles are, in fact, always the result of inflammation.

DR. PARR, 1809.—*Scrofula*.—A redundancy of albumen in early life, with an excess of oxygen and a deficiency of azote, producing a lentor in the circulating fluids.

BAYLE, 1810.—*Tubercles, Consumption*.—First employed the term "tuberculous diathesis." Designated the liability to phthisis by this name, on the principle of tubercular degeneration; that is to say, disposition to the disease. "Before this time, the theorists could not conceive any morbid deposit, suppuration, or ulceration, without preceding inflammation of the affected viscus." He denied the origin of tubercles from a pre-existing inflammation of the pulmonary tissue. Establishes two fundamental characters of consumption—the essential anatomical character and the symptomatic. The anatomical character had been neglected before him, hence the same name was given to diseases presenting analogous symptoms, but differing entirely in their nature. From the pathological appearances in the lungs he constructed six species of consumption. By the advance of pathology this arrangement has been totally revised. He distinguished

the anatomical and symptomatic differences between consumption, bronchial catarrh, pneumonia, and chronic pleurisy.

- DR. AMBRI.—*Consumption*.—An immediate but secondary effect of a necessary predisposition. The organic vice not hereditary, but children acquire from the parent the disposition to the disease. Without the predisposition, the organic vice generating the disease could not have place. From the irregularity of atmosphere and temperature arises inflammation of the respiratory organs—acute or chronic—hence that pathological condition which constitutes consumption.—(*Dic. Class. de Med.*, tom. xlv. p. 170.)
- DR. BETTOLI.—*Consumption*.—Caused by an organic performation. The consumptive person carries within himself the preorganic infirmity, ready to develope itself under a favourable concurrence of particular circumstances.—(*L. Parola*.)
- TOMMASINI, 1817-20.—*Consumption*.—The effect of a disorganizing and inflammatory process. Allows a special diathesis or dyscrasy of its own kind, apart from the inflammatory condition, which may arise from inadequacy of *stomuli*, or aliment, or a real debility, or the introduction of a poison, or an insalubrious principle, altering the quality or decomposing the liquids and solids; changing the organic mixture, the nutrition, the reproduction, and from thence corrupting the viscera and system, degenerating the tissues and humours, and leaving the individual a prey to consumption.—(*Della Inflammet e della Feb. Cent.*, t. iii., p. 164.)
- DR. BARON, 1819.—*Tubercles*.—Hydatids.
- DR. PADLINI, 1835.—*Consumption*.—Does not spare the most robust individuals, who become its victims in the vigour of life; yet there exists some cause that tends to exhaust the vital force, the organic assimilation, the hæmatisis nutrition. There is organic weakness of the patient, causing a precocious decrepitude. Therefore the tendency to consumption in robust subjects is in proportion to the strength of the preceding causes and to the severity of the inducing influence, as the abuse of liquors, unbecoming licences, and excesses of every kind.—(*Dict. Clas. de Med.*, tom. xlv. p. 343. *L. Parola, della Tuberculosi*.)
- R. CARMICHAEL, 1836.—*Tubercle*.—A variety of entozoon, having a distinct or separate animal existence. Beings possessing a vitality independent of the animal in which they are lodged.—(*On the Origin and Nature of Tuberculous and Cancerous Diseases*.)
- DR. C. CANSTATT, 1841.—*Tuberculosis*.—A degeneration of the albumen and a consequent immature and abnormal fibrine, hence a defective plasma; hence the albuminous fluids preponderate, and at length the vehicle of the albumen is unable to hold it in suspension, and it is consequently deposited in the form of tubercle.—(*Brit. and For. Rev.*, vol. xiii. p. 341.)
- GILBERT, 1842.—*Consumption*.—A want of discriminating power in the mouths of the lacteals.

PROFESSOR BUFFALINI, 1846.—*Scrofula, Tuberculous diathesis.*—

Identical. The Tuberculous diathesis, a vice of the assimilating organs arising from the scrofulous diathesis. The latter not a true morbid condition, but a particular kind of constitution predisposed to scrofula. There is an extra proportion of the earliest organic formation, *i.e.*, a prevalence of the albumen in the blood with a defect in the successive organic changes, from arrest of the respiratory process; by which arrest the albumen in excess cannot be changed into a more elevated organic principle. The diathesis consists not in a deficiency of reparative materials, but an insufficiency of that organic formation which is under the influence of oxygen—a defect of the respiratory function. Hence, in the cure, it is necessary to pay attention not to nutrition, but to a better and more exalted reparation of the oxygenated principles. He advises a free allowance of animal food; gymnastic exercises, and all those things which may contribute to a more energetic hæmatosis and to a greater development of the muscular mass.—(*Scientific Congress of Geneva*, 1846.)

DR. LANZA, 1849.—*Tubercle.*—A species of phytozoon, possessing a special anatomical and physiological existence—not a mere effusion or extravasation of a morbid element from the blood, but peculiar beings or growths, implanted on certain tissues, from which they derive their nourishment, and which they injure and destroy.—(*Brit. and For. Rev.*, July, 1849, p. 121.)

J. C. HOLLAND, 1850.—*Consumption.*—A disordered condition of the nervous system. *Scrofula.*—A peculiar state of the vital powers prone to the manifestation of certain morbid conditions characteristic of debility.—(*On the Nature and Cure of Consumption*, &c.)

PROFESSOR GIACOMINI.—*Consumption.*—A disease proper to the whole sanguineous system, consisting in an affection of the material capillary extremities of the lungs; either primary or secondary to a congenerous malady of the whole arterial system. Thus reducing the various forms and morbid products that arise in phthisis to arteritis and its events.—(*L. Parola, della Tuberculosis*.)

But possibly it may be thought that since Dr. Williams's advent, our knowledge has been vastly improved, and that *now* at least we have arrived at some definite and satisfactory solution of the matter. Let us endeavour to discover whether this be true. The following are the theories now in vogue:—

1st. *Mr. Carmichael*, in an "Essay on Scrofula," 1836, regards the disease as due to "*the generation of an acid*

in the stomach," referable to some primary disorder of digestion.

- 2nd. *Dr. Todd*, in his article on "Strumous Dyspepsia"—*"Cyclopædia Prac. Med."*—believes consumption to be due to *"congestion of the venaportæ, both in its roots and branches."*
- 3rd. *Prof. Shultz* regarded consumption as the result of the *"imperfect development of the chyle and lymph granule."*
- 4th. *Charmetton, White, Baumes, Hufeland* and others ascribe the disease to *"some peculiar modification of the lymph ;"* but what *"peculiar modification"* they never succeeded in determining.
- 5th. *M. Louigi Parola*, in 1849, advanced what I believe to be by far the most rational theory put forth, viz., that consumption results from *"defective respiration imperfectly replaced by the liver."* He *"fixes the origin and seat of consumption in the deficiency or imperfection of hæmatisis [the change from chyle to blood] in the lungs. The diminished function of the lungs results necessarily in a diminished power of sustaining the animal temperature. Hence gorged glands, vicious elaboration of chyle, and tuberculosis" [consumption].* Is it not strange that *Dr. Williams* should have sworn that imperfect respiration would not produce tubercles, knowing, as he must or ought to have done, of *Parola's* theory?
- 6th. *Broussais*, and all the followers of that great man, attribute consumption to inflammation, and regard and treat it as inflammatory in its origin and nature.
- 7th. *Dr. James Copeland* attributes the disease to some abnormal condition of the *"organic nervous influence in the first place, and the circulating fluids in the second place."* The impression is first on the *nerves*, and secondarily on the *blood*.
- 8th. *Dr. Madden* contends that the disease depends on *"a poison in the blood ;"* but what that poison may be he does not pretend to know, and chemistry has not enabled us to discover it. Still, as chemistry has

lamentably failed to detect the poison of infectious diseases, its *failure* by no means proves that such a poison does not exist. Like many Scotch verdicts, it is simply "*not proven.*"

9th. *M. Baudelocque* attributes scrofula and consumption to "*a vitiation of the blood in the process of respiration.*" Imperfection of the function of the lungs through the respiration of an atmosphere not sufficiently renewed, he regards as the *sole cause of scrofula* and tubercles. This is substantially the same as my theory. Dr. Williams distinctly swore that imperfect respiration would not produce tubercles; and yet he must also have known Baudelocque's opinions. He did not tell the Court that he was merely giving *his own* opinion, or that his opinion was disputed by much greater men. Had he done so, his Lordship and the jury would have known how to interpret his words.

10th. Dr. Hughes Bennett, the distinguished Professor of the Institutes of Medicine in the University of Edinburgh, &c., has advanced a theory which probably is the favourite doctrine of the present time, though comparatively few really understand it:—

"The peculiarity," says he, "of consumption is, that an excess of acidity exists "in the alimentary canal, whereby the albuminous constituents of food are rendered "easily soluble; whilst the alkaline secretions of the saliva, &c., are more than "neutralized, and rendered incapable either of transforming the carbonaceous constituents of vegetable food into oil, or of so preparing fatty matters introduced "into the system as will render them easily assimilable. Hence an increased "amount of albumen enters the blood, and has been found to exist there by all "chemical analysts, while fat is largely supplied by the absorption of the adipose "tissues of the body, causing the emaciation, &c. In the meanwhile, the lungs "become especially liable to local congestion, leading to exudation of an albuminous "kind, which is tubercle."

Now, clearly Dr. Williams does not believe in *this* theory, since, if he agreed with Dr. Bennett that the stomach is always too *acid*, he would certainly not try to make matters worse by his "*acid tonics.*" This would be increasing a *bad* thing, of which there was already *too much*! His treatment would feed the disease. Turning to his "*Principles of Medicine,*" I find that for excess of albumen he orders "*blood-letting, mercurials, diuretics,*

alkalies, colchicum, and low diet" (p. 172), decidedly too Sangradian for a consumptive case! It is quite clear to me that had Professor Bennett been the plaintiff in this libel suit, Dr. Williams might, with equal propriety, have sworn that there is *too little*, instead of *too much*, albumen, and hence that Dr. Hughes Bennett was, &c., &c.

Viewing Dr. Bennett's theory in its *true* light, it is in the main correct. There is no doubt an excess of the albuminous principle; but that does not arise from the causes assigned by him. Acidity of the stomach does not exist in *half* the cases until long after the disease has become established. In a large number of cases it does not occur until the last stage, and in many it *never* occurs. Besides, it is easy to account for the acidity of the stomach as a consequence of the general debility of the organism induced by the disease. Other organs besides the stomach fail in their functions in like manner.

Now if we consider that the albuminous principles of food undergo direct oxidation in the pulmonary capillaries, it is easy to understand how imperfect respiration must cause defective hæmotosis, and that imperfectly vitalized albumen is incapable of maintaining the nutrition of the body. *Emaciation* results from the loss of balance between the supply of *new* elements and the waste of the *old*. The defective albumen remains in the blood to be eliminated by some one of the excretory organs, or deposited in the lungs as an albuminous exudation of low vitality, where it undergoes fatty degeneration—the *frequent* transformation of albumen into fat being admitted by most pathologists.

Imperfect respiration thus becomes the direct cause of the mal-nutrition and the albuminous excess.

The carbonaceous waste is, in health, mainly eliminated by the lungs; but if respiration be imperfect, this function must be but imperfectly performed; hence it is that the portal vessels become engorged, the lacteals refuse the product of the liver, and the latter organ also undergoes fatty degeneration, or the excess of carbonaceous waste is eliminated by the bowels, constituting the *diarrhœa* of this disease.

This does not militate against the known fact that oils and

animal fats will cause an increase of weight in consumption. They are not supposed to undergo *oxidation* in the lungs, hence their *assimilation* does not *directly* depend on the perfection of the respiratory function. This explains the fact noticed by Dr. Ed. Smith—viz., “that patients frequently gain weight without the fatal progress of the disease being in the least arrested thereby.”

Dr. Hughes Bennet's theory admits at least of this explanation, and is not incompatible with the theory of imperfect respiration advocated by me. Still oxygen remains the chief agent in a scientific treatment, since it is only by oxygen that albumen can be raised to its normal vitality or transformed into fibrin.

I might mention many other theories, but they are really not worth the space, and besides could hardly be made intelligible to non-professional readers. What the reader will naturally expect from me, however, is that I should give the theories of the medical witnesses for the defence, that the world having lost its faith in the carbon theory, may know what it has to fall back upon.

DR. WILLIAMS' THEORY AND TREATMENT.

In his work on diseases of the chest, Dr. Williams begins by condemning the doctrines of Laennec, Andral, Chomel, Louis, and Dr. Carswell. Having thus cleared the ground, he asks a question—viz., whether tubercular deposits in the earliest stage of formation are not the product of inflammation. After having argued the point, he finally arrives at the following conclusions:—

“Without, then, going so far as to assert that the miliary indurations of the pulmonary tissue are always dependent on *chronic inflammation*, I think we may fairly say that both they and the diffused induration are more akin to the products of this process than to any other that we are acquainted with” (p. 162).

Having thus established his idea of the *cause*, he proceeds to unfold the *essential nature of the tubercular deposit*. He says:—

“Viewing tuberculous matter as a deposit of unhealthy “*fibrin* from the blood,” &c., p. 166.

This gives us his idea on that point. Dr. Hughes Bennett said it was albumen. Dr. Williams says, no, it is not *albumen*, it is *fibrin*.

Well, whether it be albumen or fibrin, let us see what he does for its cure—what is his treatment.

Regarding the disease as *inflammatory* in its origin, he begins his remedial measures by

1. “Local blood-letting,” and
2. “Counter-irritation.”—*Principles* (p. 481).

After this we come to the measures which are to be adopted for the removal of the deposits. These are—

1. “Mercury introduced by the skin.
2. “Alkalies and their carbonates, and iodide of potassium.”

Regarding which he observes:—

“Whether iodine and alkalies ever directly promote the solution or absorption “of tuberculous matter, *I am still in doubt!*”—(p. 482.)

Then he tells us that there are other agents which *do* produce decided effects:—

“The most effectual solvents of all these constituents are caustic alkalies.” . . .

“*But it is impossible to administer these agents in sufficient quantity to obtain their influence through the blood, both because they would irritate the stomach and vessels too much during their passage, and because they would soon meet with such an amount of acid (particularly the carbonic) as would deprive them of the greater part of their solvent power.*”

So, of course, they can do no good, but might be prejudicial. Then comes:—

“Other agents *supposed* to act by dissolving the *fatty* constituents of “tubercle, and in that way promoting their disintegration. Of this class are “naphtha, pyro-acetic spirit, oil of turpentine, tar, sulphuric ether, and various “fixed oils” (p. 483).

These he admits may add to the mischief rather than remove it, checking expectoration, exciting pain and tightness of the chest, and hard cough, which may end in inflammation or hæmorrhage! So they too may be passed over as worthless.

Next he comes to his favourite remedy,

COD-LIVER OIL,

which he thinks may do good, but is "apt to disagree in cases of inflammatory dyspepsia, especially that affecting the duodenum; in those of liver affections with fulness and tenderness," &c. These should be *first* reduced by

Mild Mercurial Aperients.

Having explained how the oil is to be taken, and that the mercurials may be "*frequently required during its use*," he comes to those means which have a tendency to favour the "*absorption*" of the tubercles. He notes the effects of exercise on the circulation, which he believes

" promotes the gradual solution of the deposits chiefly by the *oxidizing* influence of the current of *arterial* blood that is thrown with more force through the neighbourhood. This probability naturally suggests the inquiry whether it be possible to aid this process by *oxygenating the blood more highly* than can be done by the free respiration of the pure air! I have already suggested that it is probable that such agents as nitric and nitro-muriatic *acid* and *chlorate* of potassa may contribute to this object. The oxygenating medicines also sometimes produce decided improvement in the general and local symptoms of scrofulous and tubercular disease during the maturation and "softening of their deposits" (p. 492).

This proves conclusively that he thinks there is too much *carbon* in the blood, and that the blood needs more *oxygen*.

A summary of his treatment is as follows:—

1. Local blood-letting.
2. Counter irritation.
3. Cod-liver oil with "mercury."
4. Acid tonics.

The local blood-letting I will venture to say he has abandoned in practice. Cod-liver oil, he admits, often disagrees, and when it does he falls back on *mercury*. Lastly, he tries to *oxidize* the blood by giving medicine which contain "*much oxygen in loose combinations*." Though he said chloric acid in the lungs would not give up oxygen to the blood, he thinks *chloric acid* and potash *will* do so through the stomach! while even *chlorine*—that deplorable irritant—is a good thing if given

in his way, but *not* if given in mine. Then *why* does *he* want to *oxygenate* the blood more highly if, as that "very intelligent chemist" says, it has already got too much oxygen? It was a great oversight on his part that he did not make the chemist read his book before he gave his evidence. Either Dr. Williams or the chemist, or both, have been *slightly* practising upon the *credulity* of the public!

DR. COTTON'S THEORY AND TREATMENT.

This gentleman has written a book upon consumption, though *why* he did so I confess myself utterly unable to comprehend, unless a confession of his ignorance on every material point is to be regarded in the light of a *penance* imposed upon him by the spirits of his departed patients. Hear what *he* says:—

"The term consumption is now universally and exclusively applied to a *peculiar and obscure condition* of the whole system, in which, instead of the healthy "nutritive material required for the growth and reparation of the body, there is "produced in the blood a morbid substance which sooner or later appears as "tubercle or tuberculous matter in the pulmonary structures" (p. 2).

Now, what "*a peculiar and obscure condition*" is, is precisely what we desire to discover. His definition is *obscure* enough certainly, and it is a very *peculiar* circumstance that a man who has no clearer ideas should have deemed it necessary to write a book to *enlighten* other people. But perhaps he will be able to tell us what *tubercle* is:—

"Of the intimate nature of this diseased condition our knowledge is indeed "*very limited*! In consumption, as in many other maladies, we are permitted to recognise the disease only in its effects. Of the causes from "which these may spring *we are completely ignorant*. It is evident that there "must be *something* which constitutes the malady, but it would be vain to search "after it" (p. 3).

It certainly is evident that there must be *something* the matter, when the lungs are stuffed with tubercle, and before a physician presumes to prescribe for its removal, he had better have *some* idea what that "*something*" is. So long as he is "*completely ignorant*," he is in very much the same condition as the patient himself, for he certainly *believes* there is *something* wrong. If the physician has any definite idea what

that something is, he ought to say so, and if he has no fixed idea, he ought to abandon the practice of medicine altogether. How is it possible for him to prescribe for the cure of a disease concerning the very nature of which he has no settled convictions? His remedial means cannot rise above a mere treatment of symptoms. No wonder after this confession that he tell us

"In England and Wales more than one hundred and forty-three persons die "daily of consumption, being very nearly at the rate of six deaths in every hour, "or one in every ten minutes!" (p. 4.)

Surely this passage must be regarded as a splendid specimen of "terrorism." Why did he put that in his book I should like to know? Was it for the benefit of the medical profession, or for *public* cogitation? When we consider that Dr. Cotton's book was advertised in the *Times*, and was no doubt purchased and read by many poor suffering invalids, what so natural as that they should immediately rush off to consult the learned, and benevolent physician, who had discovered that one person dies from this disease every *ten minutes*, six in every *hour*, and more than *one hundred and forty-three daily*? Hear what he says about "The Curability of Consumption."

"At the *very earliest* period at which consumption is recognisable . . . much may "oftentimes be effected by properly directed remedial measures; and numerous "cases which, if but for a short time neglected, would pass on to the obstinate "stage of tubercular deposit, may, I am convinced, be completely restored."

That certainly is comforting to such as are in the "*very earliest*" stage, but quite otherwise for those who have gone a step beyond it. If there is little or nothing the matter, go to Dr. Cotton and he may help you to get rid of it. If there is anything serious the matter—well, read the above passage and those which I have previously quoted, and then—do what you please. But bear in mind that in his evidence he stated that "shortness of breath, cold hands and feet, a frequent pulse and a seated dry cough," were not evidence of sufficient danger to require you to have your chest examined. Never mind such symptoms. Dr. Hunter is only trying to alarm you. Let them go on, and then see whether your disease will be discovered and treated at "*the very earliest period.*"

We now come to the treatment ; of this he makes two divisions.

1. *Treatment before Tubercles.*

“It is at this very early period—the preliminary stage or dawn, as it may be termed, of consumption, that the success of proper treatment is the most frequent “and conspicuous” (p. 195).

In other words his treatment is most *successful before* the disease is set up. When the lungs are perfectly sound and free from tubercles, he can *cure* consumption ! There is, I think, no doubt, that his treatment, if it ever does cure consumption, is most likely to do so before there is any disease !

Treatment after tubercles.

“All hope need not vanish. It has already been stated that the tubercular deposits *may*, in *some* cases, be entirely absorbed” (p. 201).

The indications to be fulfilled by remedial measures are threefold, as follows :—

- (1.) To restore the health and vigour of the body.
- (2.) To relieve the different symptoms.
- (3.) To check the further progress of the local or pulmonary disease.

(1.) The first of these is to be attempted by stimulants and tonics, “among which fish-oil, iron, mineral acids, and bitters,” he tells us “are the chief” (p. 208).

Now, if these things would “restore the health and vigour “of the body,” it would be all very well, but alas ! experience proves that, notwithstanding Dr. Cotton has been prescribing them for twenty years, “more than 143 die daily—*six* in every “hour, and *one* in every ten minutes !”

(2.) “The second object is to be met by *appropriate* medicines ; but as none of these possess any specific action, the “choice must be left to the practitioner.”

“For the cough there are the various combinations of ipecacuhana or squills, “conium, hyoscyamus, with tragacanth, spermaceti, or linseed.” . . . “Nothing, “however, so readily relieves the distressing cough, which is often met with in “consumptive cases, as *morphia* (opium) (p. 209).

Then follow stimulating liniments or a small blister. . . . Inhalation is something beneficial, and may be performed by means of any of the numerous inhaling instruments, or by a

funnel inverted over a basin of water.* . . . "A sedative *may* contribute to its efficacy—from ten to thirty grains of the extract or a drachm of tincture of conium or hyoseyamus, or from five to fifteen drops of diluted *prussic acid*" (!)

For spitting of blood he advises "lead and opium," or "sulphuric acid with alum," while bleeding "from the arm *may* be serviceable."

(3) Now we come to the third and last indication—viz., "to check the further progress of the lung disease." This certainly is a very important object, but alas! we are again, I fear, to be doomed to disappointment. He says it—

"May be frequently carried out with considerable success by means of counter-irritants applied to the chest. It cannot, of course, be expected that any local treatment will certainly *prevent* the further formation of the tubercular deposit" (p. 220).

Then he talks about treatment of the second stage; but if he could do no more than is indicated in the first, the second stage is almost a necessary consequence, and I fancy the reader will hardly care to go further. Still, as we have gone so far, we may as well see the end, which is near at hand.

"For the delirium which attends the last hours of the sufferer little indeed can be done. Occasionally it seems to be connected with the administration of *opiates*. . . . But at this solemn period neither opiates nor any other narcotic should be given that might interfere with that mental life, which, in bidding adieu to this world, sometimes presents a brilliancy which, however transient, is sometimes usefully employed by the dying and thankfully remembered by the living!"

I have now conducted the reader to the termination of Dr. Cotton's treatment, which he will observe is very touching and pathetic, as well as *suggestive*. The whole of his method may be summarized as follows:—

- 1st. If there is no disease I can probably cure you.
- 2nd. If the disease is in its very *earliest stage* I may possibly do so. I think I have known *some* in the last 20 years who did get well.
- 3rd. If the disease has reached a decided stage make the patient as comfortable as you can, and stop the opium when the final end approaches.

* Certainly a *little more* scientific than Dr. Williams' towel arrangement.

His remedies are

“Cod-Liver Oil,”
 “Cough Mixtures,”
 “Tonics,” and
 “Counter Irritation.”

Under the curative operation of which “143 die daily, six in every hour, and one in every ten minutes!”

Drs. Markham, Johnson, Risdon Bennett, and Odling, unfortunately for mankind, have not written any guides to health. With great discretion they have avoided committing their opinions, if they have any, to the world. Medical literature has not been enriched by *their* experience in consumption. Still they are all men of mark. Dr. Bennett has published a book on hydrocephalus, in which he informs us he had treated *four* cases of *that* disease, of which he made *post mortem* examinations. Dr. Johnson has recently written a pamphlet on cholera, which he proposed to treat by *castor-oil*, but I believe the system has since been generally abandoned for the reason that the patients were so absurd as *not to recover* under its operation.* Dr. Markham, I must not forget, has to a *limited* extent, placed his opinion as to the nature of consumption upon record. It will be found in the *Medical Times and Gazette* for 1857, and is as follows:—

“Of course we all know well enough that there is *a* morbid condition of the “body existing anterior to the deposit of tubercle in the lungs. . . . And we “know also that treatment is not directed to the tubercle, but to the averting, *it* “*may be*, of that morbid condition which presides over the deposition.”

Yes, “*it may be*” that the treatment *does* tend to avert that morbid condition, but since he evidently does not pretend to have any clear idea what “*that morbid condition*” is, it *may be that* it tends to *increase* rather than to avert it.⁴ “A morbid condition” is simply a confession of ignorance, and a form of expression used to avoid giving any opinion whatever. It may mean anything or nothing, and yet I have no doubt it very fairly conveys the extent of Dr. Markham’s knowledge.

* Dr. Owen Rees and the authorities of Guy’s Hospital have recently exploded the doctor’s theory and treatment of cholera in a summary manner.

THE DEATH TABLES

OF

ENGLAND AND WALES.

I PROPOSE now to interrogate the *statistics* of England and Wales, to ascertain whether British physicians be really endowed by nature, as they fain would lead us to believe, with some *special* gift of healing, whereby they become of *higher* skill than the physicians of other countries on a *lower* education. If we shall find that the death rate in England is lower than in Canada and the United States, and that it is less now than it was twenty-five years ago, it will go far to reconcile us to their diplomas, and tend to *palliate*, if it does not justify, their bad habit of throwing stones at their neighbours' windows.

We know that we are all exposed to great perils from disease, and that although the allotted age of man be "threescore years and ten," but few of us can hope to escape the toils of the destroyer to the full period of our days. Diseases threaten us on every side, and thence it is that *death tables* are valuable as revealing to us those maladies which cause the chief destruction of human life. Mankind cannot know too much about the *nature* of these maladies, nor of the *causes* by which they are engendered, or the best means of their *prevention* and *treatment*. To aid the reader I shall introduce each table with a brief explanation of what I conceive it proves, but still leaving him to exercise his own judgment. I

will take the tables of the Registrar-General as a basis, and select from them those facts which show us the mortality caused by particular diseases, and the gross mortality from all causes in a given population. By comparing these with the mortality of twenty-five years ago, we shall see whether the present generation of English doctors is more or less skilful than their progenitors. If fewer people die in each 1,000 of the population now than did twenty-five years ago, that fact will be something in their favour, but if on the other hand *more* die, it will prove that all their boasted progress is vain and delusive.

It is desirable that the reader should keep constantly in mind *results actually obtained*, and that he should judge of the present by comparing it with the past. Great changes have certainly taken place in Medical practice. A physician might to-day be arraigned for malpractice, and convicted by the authority of the schools, for *doing* what he would have been arraigned and convicted for *not* doing twenty years ago. The wholesale use of "calomel," the "leeching," "bleedings," and "black draughts" which twenty years ago were standard remedies in orthodox practice, and on the assumed virtues of which every physician relied for the cure of the sick, are now all abandoned as unnecessary, if not positively injurious, and their place has been supplied by newer conceits and newer concoctions of the Medical brain. What we have to decide is, not whether the former practice was *correct*, but whether that at present in fashion (for there is as much fashion in physic as in furbelows) be *better* or *worse*. If more successful, then mankind has gained something by the change; if less, then it has lost. The death tables for England and Wales will tell us how the matter stands, and to determine this is the object of the present investigation.

TABLE I.

The three columns of figures in the following table show us—1st, That the population of England and Wales has increased in a certain ratio; 2nd, That the deaths from all causes have increased in a ratio greater than the population; and 3rd, That the deaths from diseases of the respiratory organs have increased in a ratio greater still.

TABLE I.

Showing the Population and aggregate number of Deaths from all causes, and the number of Deaths from Diseases of the Respiratory Organs, in England and Wales in the years 1838 to 1864.

Years.	Population.	Deaths from all causes.	Deaths from Diseases of the Respiratory Organs.
1838	15,312,256	342,547	90,823
1839	15,515,296	338,979	90,565
1840	15,721,092	359,561	92,907
1841	15,929,492	343,847	92,183
1842	16,123,793	349,519	92,994
...	No records were kept during these five years.		
1848	17,340,492	399,800	97,077
1849	17,552,020	440,853	95,300
1850	17,766,129	368,986	91,918
1851	17,982,849	395,174	97,994
1852	18,193,206	407,138	98,385
1853	18,404,368	421,104	111,777
1854	18,616,310	437,916	94,113
1855	18,829,000	425,703	98,274
1856	19,042,412	390,506	116,406
1857	19,256,516	419,815	98,274
1858	19,471,291	449,656	108,911
1859	19,686,701	440,781	116,551
1860	19,902,713	422,721	121,015
1861	20,019,314	435,114	127,063
1862	20,336,467	436,566	122,300
1863	20,554,137	473,837	125,753
1864	20,772,308	495,531	128,800

The importance of diseases of the organs of respiration will be understood from the fact that they cause a *fourth* part of the gross mortality of this country. Nay, if we deduct from all causes the deaths occurring in infants at and immediately subsequent to birth and those caused by accidents and violence, we shall find that diseases of the air passages and lungs cause in England and Wales nearly a *third* of the deaths which result from disease.

What, then, do we learn from this table? It informs us that, whatever may have been the changes in medical practice during the last 25 years, they have not resulted in any diminution in the relative number of deaths. It reveals that, on the contrary, there has been a slow but steady *increase* in the aggregate mortality, and a more decided increase still in the fatality of Pulmonary maladies.

TABLE II.

The following table exhibits the annual deaths caused by *nine* of the most important diseases of the Organs of Respiration from 1838 to 1864, and enables us at a glance to form a correct estimate of their relative fatality.

It has been gravely claimed that the decrease observable under the head of *Phthisis* affords proof of increased success in the treatment of this disease. But is this decrease real? Glance at the second column and you will at once see how it is accounted for. In 1848 a reduction of **12½ per cent.** appears to have been effected from the corrected average of previous years, but mark that in the very same year there is a suspicious increase of *seven hundred per cent.* in the deaths from *Bronchitis*! Prior to 1848, the yearly mortality from Bronchitis ranged between **1,663** and **2,627** (these being respectively the

highest and lowest numbers of the five years preceding), but in that year it rose to **14,472!** The population of England and Wales in 1848 was 17,340,492, or scarcely $3\frac{1}{2}$ millions less than in 1864. Now look at the relative mortality of these years. To have kept pace with the increase of population the deaths from Bronchitis should have been **17,061** in 1864, instead of which they attained the astounding total of **38,969**, or 21,908 above the corrected average. By following the figures under the head of Bronchitis from the year 1848 down to 1864, it will be seen that the *suggestive* fatality which characterised the former year has not only been maintained since, but has increased from year to year with fearful rapidity.

Under the third head, "Lung disease" (which is only another term for Phthisis or Consumption), we find an increase of 81 per cent. in the 27 years from 1838 to 1864, against an increase in the population of $29\frac{1}{4}$ per cent., while under the head of "Laryngitis" there is an increase of more than *fourteen hundred per cent.* for the same period!

TABLE II.

Showing the number of Deaths from particular diseases of the Respiratory Organs in England and Wales in the years 1838 to 1864.

Years.	Phthisis.	Bronchitis.	Lung Disease.	Pneumonia.	Asthma.	Quinsy.	Pleurisy.	Laryngitis.	Diphtheria.	Total.
1838	59,025	2,067	2,067	17,999	5,745	432	582	99		88,016
1839	59,959	1,663	2,661	18,151	5,183	659	588	62		88,926
1840	59,923	2,053	2,737	18,582	5,779	680	702	106		90,562
1841	59,592	2,267	2,788	17,997	5,976	505	675	101		89,901
1842	59,291	2,627	2,944	19,036	5,625	513	729	102		91,867
...
1848	51,713	14,472	2,645	21,862	3,920	569	1,029	867		97,077
1849	50,299	14,826	2,604	21,194	4,104	459	956	858		95,300
1850	46,618	14,611	2,409	20,303	4,574	473	877	1,053		91,918
1851	49,166	17,294	2,645	22,001	4,896	369	984	939		97,994
1852	50,594	17,073	2,569	21,421	4,309	391	945	1,083		98,385
1853	54,918	22,391	2,852	24,098	5,143	421	855	1,097		111,777
1854	51,284	20,062	2,528	23,523	4,271	345	955	1,145		94,113
1855	52,290	27,182	2,746	26,052	5,454	374	1,153	1,155		116,406
1856	48,950	21,528	2,444	22,653	4,103	416	886	1,294		98,274
1857	50,106	25,588	2,707	23,457	4,339	485	870	1,359		108,911
1858	50,442	29,093	3,139	26,486	4,513	623	846	1,439		116,581
1859	50,149	25,998	2,882	24,514	4,224	426	916	1,319	9,587	120,015
1860	51,024	32,347	4,424	25,264	4,325	319	882	1,166	5,312	127,063
1861	51,931	30,986	4,484	22,914	3,892	342	781	1,253	4,517	121,300
1862	50,962	32,526	4,928	23,713	4,087	323	833	1,478	4,903	122,753
1863	51,072	32,025	4,907	24,181	3,639	334	907	1,561	6,507	125,393
1864	53,046	38,969	5,158	24,470	4,228	378	941	1,610	5,464	144,264

No record kept during these five years.

Record of this disease not kept during these years.

How are we to account for these amazing facts, or for the silence of the Medical Profession and Medical Press of this country in regard to them? Is it possible that they have been entirely overlooked, or were they

observed but not thought sufficiently indicative of *medical progress* to require further investigation?

TABLE III.

The tables following have been constructed for the purpose of comparing *five* years, from 1838 to 1842, with *five* years from 1860 to 1864, and show—1st, The population of the two periods; 2nd, The deaths from all causes for the two periods; 3rd, The deaths from diseases of the Respiratory Organs for the two periods; 4th, The aggregate deaths in each 1,000 of the population; and 5th, The comparative mortality from diseases of the Respiratory Organs.

The lower table gives at a glance the *numerical* and *per cent.* increase which has taken place in the population, in the deaths from all causes, and in deaths from pulmonary diseases.

It will be seen that during the first period the average yearly deaths from all causes were **346,890** in a population of **15,720,391**, and the deaths from diseases of the Respiratory Organs **91,894**; that in each **1,000** persons a fraction over **21** persons died from “all causes,” and a fraction over *five* from diseases of the Lungs.

In the second period the average annual deaths from all causes were **452,754** in a population of **20,316,987**, and from diseases of the Respiratory Organs **125,061**; in each **1,000** persons a fraction over **22** persons lost their lives by “all causes,” and a fraction over **6** died of disease of the Lungs. The deaths *increased* from all causes from $21\frac{1}{2}$ to $22\frac{1}{4}$ in each thousand souls, and from disease of the Lungs from $5\frac{3}{4}$ to $6\frac{1}{4}$. The increase of the population was therefore $29\frac{1}{4}$ per cent., deaths from all causes $30\frac{1}{2}$ per cent., and deaths from diseases of the Respiratory Organs $36\frac{1}{10}$ per cent.!

TABLE III.

Showing the increase of Population, and the relative increase or decrease of Deaths from all causes and from diseases of the Respiratory Organs in England and Wales in 5 years from 1838 to 1842, and in 5 years from 1860 to 1864.

In Five Years from	Average Population.	Average Deaths from all causes.	Average Deaths from Diseases of the Respira- tory Organs.	Deaths in each 1,000 persons living from all causes.	Deaths in each 1,000 persons living from Diseases of the Respiratory Organs.
1838 to 1842	15,720,391	346,890	91,894	21½	5¼

In Five Years from	Average Population.	Average Deaths from all causes.	Average Deaths from Diseases of the Respira- tory Organs.	Deaths in each 1,000 persons living from all causes.	Deaths in each 1,000 persons living from Diseases of the Respiratory Organs.
1860 to 1864	20,316,987	452,754	125,061	22¼	6¼

—	Population.	Deaths from all causes.	Deaths from Diseases of the Respira- tory Organs.
	Numerical Increase Per Cent. Increase	105,964 30½ (nearly)	33,167 36½
	4,596,596 29¼		

TABLE IV.

The following table illustrates the increase of deaths from five diseases of the Respiratory Organs. By

comparing five years from 1838 to 1842 with five years from 1860 to 1864, keeping in mind the difference of population for the two periods, we shall be able to fairly estimate the actual increase which has taken place in each disease.

The *whole number* of deaths caused by Bronchitis in the *five years* ending 1842 was **10,677**. The average population of this period was **15,720,391**. During the second period of five years ending 1864, the average population was **20,316,987**, or a fraction above $4\frac{1}{2}$ millions increase. A correction for difference of population would give us for the second five years **13,776** deaths from Bronchitis—supposing the ratio of mortality to have been the same. But what do we find was the *real* mortality from this disease? Why, that instead of **13,776** it attained the enormous total of **166,953**, or an increase of more than *fourteen hundred per cent.* above the corrected average. Prior to 1842, Bronchitis caused in five years only **679** deaths in each *million* of the population, while in the period from 1860 to 1864 it destroyed no less than **8,222** *in the million*!!

The second disease in Table IV. is a curiosity of classification. Surely the medical men who filled up the burial certificates ought to have known whether the cause of death was Consumption, Bronchitis, Asthma, or Pneumonia? It would be scarcely less absurd to certify that a person died of *disease of the body*! All we know is that in the former period **13,197** deaths occurred in England and Wales from some form of pulmonary affection, which deaths were certified as arising from “**LUNG DISEASE.**” (!) A correction for difference of population should have given us, at the same rate of mortality, **17,596** deaths from “**Lung disease**” for the second period, instead of which there were **23,901**, or more than *six thousand* above the estimated average.

TABLE IV.

Showing the increase of Mortality from certain diseases of the Respiratory Organs above the ratio of Population, comparing 5 years from 1838 to 1842 with 5 years from 1860 to 1864.

First 5 years. *Estimated average Population 15,720,391.*

Diseases.	In each year.					In the 5 years.	Proportion of deaths from each disease to 1,000,000 persons living.	Aggregate proportion of deaths to one million persons living.
	1838	1839	1840	1841	1842			
	1838	1839	1840	1841	1842			
Bronchitis	2,067	1,663	2,053	2,267	2,627	10,677	679.17	7,594.4
Lung disease ...	2,067	2,661	2,737	2,788	2,944	13,197	839.5	
Pneumonia ...	17,999	18,151	18,582	17,997	19,036	91,764	5,837.25	
Pleurisy	582	588	702	675	729	3,280	208.6	
Laryngitis	99	62	106	101	102	470	29.9	

Second 5 years.

Estimated average Population 20,316,987.

Diseases.	In each year.					In the 5 years.	Proportion of deaths from each disease to 1,000,000 persons living.	Numerical increase.	Per cent. increase.
	1860	1861	1862	1863	1864				
	1860	1861	1862	1863	1864				
Bronchitis ...	32,347	30,986	32,526	32,025	38,969	166,953	8,222.3	156,276	1,463.6
Lung disease	4,424	4,484	4,928	4,907	5,158	23,901	1,176.4	10,704	81.1
Pneumonia .	25,264	22,914	23,713	24,181	24,470	120,542	5,933.5	28,778	31.3
Pleurisy	882	781	833	907	941	4,344	214.6	1,064	32.4
Laryngitis...	1,166	1,253	1,478	1,561	1,610	7,068	347.8	6,598	1,403.8

In *Pneumonia* and *Pleurisy* the increase is only small, and hence of no great importance.

Laryngitis, on the other hand, increased enormously.

In the first *five* years there were only **470** deaths from this disease, while in the second they amounted to **7,068**. Only **29** persons in each million of the population were cut off by Laryngitis in the former period, while **347** in each million were destroyed in the latter. These are startling facts. Of late years it has become fashionable to torture the poor larynx by strong caustic applications, and to facilitate their employment an instrument called the "**LARYNGOSCOPE**" has been invented and widely introduced into practice. It is at least suggestive that since its introduction this disease should have so frightfully increased in frequency or fatality!

The per cent. increase in these diseases against an increase in population of $29\frac{1}{2}$ per cent. is as follows:—

Bronchitis	1,463	per cent.
Lung disease	81	„
Pneumonia.	31	„
Pleurisy	32	„
Laryngitis	1,403	„

No man capable of reasoning can reflect upon the facts contained in the foregoing tables without arriving at the conclusion that, whatever may be the case "in Chili and Peru," there is something radically wrong in the prevailing practice of medicine in England! The population increases, the condition of the poor improves, and yet, as if to proclaim the fallacy of medical theories and the imperfection of British medical practice, Death gathers yearly his victims by increasing numbers! How are we to reconcile this fact with the assumed superior knowledge and the vaunted superior skill of English practitioners?

The reports of the Registrar-General demonstrate that many diseases which a few years ago were regarded as simple in their nature and perfectly curable, have become of late alarmingly fatal; and this too at the very time the Medical Journals are

boasting most loudly of "the progress of Medical Science." These tables of the Registrar-General are founded upon the *certificates* of the *registered* Medical men of England. If the tables are erroneous, the fault lies with *those who furnished* the certificates; if the tables are correct, then is the profession retrograding rather than advancing in practical skill.

Take, for example, two diseases of the Respiratory organs—Bronchitis and Laryngitis. How are we to account for the astounding increase in the deaths which have been occasioned by them in late years? Are there *fourteen* times as many cases of such diseases? Have these diseases become *fourteen* times more fatal? or have the doctors been *doctoring the statistics*? These are questions which will naturally suggest themselves to the mind—questions which the medical profession of England only can, and which they certainly ought to, answer.

If it is pretended that this apparent anomaly is to be accounted for by a mere change of name from *Consumption* to *Bronchitis*, then why make such a change, one especially calculated only to produce a false impression on the public mind in regard to the actual fatality of the former disease? It cannot be claimed that it arises from improved *diagnostic* skill, for we find a very large increase in the number of deaths recorded as having occurred from "*Lung disease*"—an entry which would rather suggest the thought that there is an increased number of physicians who are unable to diagnosticate at all—men who, notwithstanding their English education and English diplomas, cannot distinguish one form of lung disease from another, and hence cloak their ignorance under this general term.

No amount of arithmetical philandering will enable English physicians to conceal the damaging fact that a larger number of people now die in each year in

every thousand of the population of England and Wales—that is to say, that the death rate is numerically higher than it was twenty-five years ago. The climate is the same, the surroundings of the poor are more favourable to health and longevity, the profession claims to be more enlightened, while it certainly possesses more extensive medicinal agencies; yet, notwithstanding this combination of favouring circumstances, more patients die under their hands. If this be a proof of the “progress of Medical Science,” then assuredly what is *called* Medical Science must be some abstract idea entirely unconnected with the healing of the sick.

Twenty-five years ago a fraction over 21 deaths occurred in each thousand of the population, and *now* something over 22 deaths occur. *Then* a fraction over 5 died from disease of the air passages and lungs, and *now* more than 6 die from the same maladies. Are these signs of progress?

In twenty-seven years the population of England and Wales increased $29\frac{1}{4}$ per cent. Now had disease of late years been treated with *exactly* the same success as formerly, the proportion of deaths from all causes would have maintained the same ratio of increase as the population. Had the treatment of disease become *more* successful, the percentage of mortality would have been absolutely *less*. But when we analyse the death tables, we find that the death rate has increased from $29\frac{1}{2}$ to $30\frac{1}{2}$ per cent.; while in diseases of the air passages and lungs this rate has actually increased to $36\frac{1}{10}$ per cent. Can we have a more withering proof of the low standard of medical skill in England?

The British profession, in libelling the medical men of other countries, have fairly exposed themselves to reprisals, and must not blame us if, in self-defence, we expose the folly of their pretensions by

appealing to their works. The liberality of the English people has provided splendid hospitals for the treatment of diseases of the chest, yet what have these costly establishments become but *homes* for poor languishing invalids to die in? Their "reports" tell us of "discharges" "incurable" and "palliated," but we search them in vain for a record of those who have been *cured*.

What do English physicians know of the nature of pulmonary diseases which they did not learn from *foreigners*? Where did they gain a knowledge of *percussion* but from the labours of Avenbrugger? Where did they learn the art of *auscultation* but from the writings of Laennec? To the French and German schools they are indebted for the system of *physical diagnosis*, and for very much of the *pathology* of tubercular disease, while even "*Cod Liver Oil*," on which they almost exclusively rely in its *treatment*, was picked up in a German Hospital! How is it, then, that they affect to regard French and German physicians as inferior? No liberality on the part of foreign nations, no generous concessions made by the colonies, can win either *justice*, *courtesy*, or *fraternity* from the British profession.* Their insular *pride*, *prejudice*, and overweening *self-conceit*, the offspring of centuries of *monopoly* in union with

* More than TEN THOUSAND British physicians have in the past forty years found homes, employment, and a fraternal welcome in the United States and Canada. Their English diplomas have been recognised as entitling them not only to *practice*, but to perfect equality in *rank* and *privileges* with American and Canadian physicians. How has the British profession returned this liberality? By libelling the medical institutions of Canada and the United States; by refusing to recognise their diplomas, and by praying Parliament to enact a law to deprive the few (perhaps 15 or 20) American and Canadian physicians now residing in England of their collegiate degrees and the privilege of *practice*!

profound ignorance of everything outside the bounds of these islands, make them the pariahs of Medicine.*

During the late visitation of the Cattle Plague it was amusing to see the zeal and flourish of trumpets

* Some two years ago a distinguished American physician, Dr. Marion Sims, came to reside in London. He had been for years connected with a New York hospital, was an able contributor to the literature of the profession, and had won a world-wide celebrity by important operations and improvements in the surgical treatment of certain diseases of women. Before coming to England he had resided a short time in Paris, where he was admitted a member of the profession, received the Red Ribbon of Honour from the hands of the Emperor, and had been called in consultation to attend upon the Empress. I mention these facts merely to show that his reputation was among the highest. Now what was the treatment he received from the British profession? He was denied *registration*; he was *lampooned* by letters in the Medical Journals, and, lastly, the editor of "*The Medical Times and Gazette*," under pretence of *noticing* a book *designed exclusively for the profession*, assailed him as follows:—

"We shall not be surprised, by the way, if the publication of this book, and the extension of the remedial agent for sterility which it includes, shall in time supply a great want, in the shape of a new, elegant, truthful, and suggestive term of vituperation. We are already in possession of a tolerably copious vituperative vocabulary, based upon the supposed paternity or maternity of the person vituperated, and varying from the mild 'son of a gun' up to the more sporting phrase, 'son of a bitch,' and to the still severer 'son of a w—e.' If Dr. Marion Sims's book acquire due vogue, young people in the next generation will ask in wonder the meaning of the phrase 'son of a squirt.'"—*Medical Times and Gazette*, Feb. 3, 1866.

Whoever fills the editorial chair of a prominent Medical Journal in England is naturally supposed to reflect in some degree the *spirit* and *moral tone* of the British profession. Has the editor of "*The Medical Times and Gazette*" done this? Is he to be regarded as one of the *gentlemen* of which it is composed? Possibly, after all, the case is exceptional and was entirely due to the fact that Dr. Marion Sims was *only* an American physician, and as such entitled to scant courtesy. However it may be accounted for, the fact remains. Dr. Sims immediately shook the dirt from off his feet, and left London.

with which leading medical men invaded the province of the *Cow Doctors*—the announcement being duly heralded by letters in the public newspapers; but, after all, what (beyond the acquisition of a cheap notoriety) did they accomplish towards mitigating the ravages of the disease or relieving the sufferings of the poor brutes? They simply counselled them to be “*slaughtered!*” What have they accomplished in Cholera? They have given us, it is true, a dozen different theories of the nature of the disease, and a score of different remedies, having little or no analogy in their action on the system, but are compelled to confess themselves as much in the dark as ever. Nothing has been discovered which does any real good. After *three* visitations of Cholera, and nearly *half a-century* of investigation, they are still in the clouds of uncertainty, doubt, and theoretical speculation as to its nature, while in its treatment old women and “sea captains,” armed with “*salt and red pepper,*” contend with the schools, and seem not unlikely to carry off the palm of victory!

In order to estimate the discrepancy between *theory* and *practice*, we have only to take for illustration the various *infectious diseases*. It is an established doctrine that these diseases arise from a specific poison which floats in the atmosphere and is inhaled with the breath. It is admitted that whoever inhales this polluted air becomes thereby liable to the disease. It is also admitted that any poison so inhaled *goes directly into the blood* and produces its action wherever the blood circulates, or in other words in every part of the body. Now if this be a correct doctrine, surely common sense teaches us that to cure such a disease we must *neutralise the poison* in the blood, and that the channel by which it entered the system—*i.e.*, the lungs—is the proper one by which to convey the *antidote*. If the poison could be inhaled, certainly

neutralising and purifying gases can also be inhaled, and just as the poison acted upon the blood injuriously, so these would act upon it beneficially. All this is plain, and ought to be intelligible to the meanest understanding, and yet, strange as it may seem, it has never been acted upon in the treatment of these diseases.

To endeavour to detect the nature of the poison is the first step in a *scientific* investigation of these maladies, and yet it is a step which has not yet been attained in regard to any form of infectious disease. To *neutralise* the poison in the blood before it has produced disorganisation in the solids and fluids of the body, which no human agency can remedy, *ought* to be the first step in a scientific treatment, and yet it has never even been *attempted*. No, medical men are satisfied with the administration of crude medicines by the stomach, which as often do *harm* as good, after which they fold their arms and await the result. If the poison subdues nature and kills the patient, the doctor's conscience is quieted by the knowledge that he has done all that the books recommend.

Most people no doubt conscientiously believe medicine to be a *science*. It is nothing of the kind, but, on the contrary, an imperfect *art*, full of errors, at which the next generation will laugh as undeserving the respect of rational beings. We may see these errors, but it is a lifelong work to remove them. The mass of mankind are wholly ignorant of the simplest laws of health. There is no public opinion to appeal to. The discoverer of an important medical truth is regarded by the whole body of the profession, from the moment of its announcement, as an enemy, for he purposes not only to overthrow the prevailing error, but to wreck the reputations built upon it. Medical men cannot admit that he is *right* without

confessing that they are *wrong*. We must be much nearer the *millennium* before they will do this. They may not be able to oppose the new doctrine in argument, but they can unite their voices to slander the *discoverer* out of his reputation. Well might Sir Isaac Newton, weary of the strife and indignant at the misrepresentation to which he was exposed, declare that, had he his life to live over again, and was so *unfortunate* as to make discoveries, he would leave them to come out after him, for he found that the peace of his days was destroyed by the slander of his enemies.

Interest, pride, and vanity in the medical profession are the chief obstacles to the progress of the Art of Medicine. So long as the people are satisfied, and believe with the poet that—

“Whatever is, is right,”

cholera and consumption, and many other diseases, will remain as they are now the *opprobrii medicorum*—no change will be made in their treatment, no scientific effort be made to discover their nature or to effect their cure. *Fish-oil* will bound the remedial means employed in the latter, while possibly for the former the “*stamping out*” plan, adopted in the Cattle Plague, may come into orthodox favour!

In conclusion, with every disposition to be just towards *English doctors*, I am compelled to state, as the result of my investigations, that they cannot appeal to the *statistics* of their country in proof of their skill.

Believing that the facts I have collected in these researches may prove beneficial, I cast them forth upon the world. If they do no other good they will at least teach the profession of this country two virtues which they are sadly in need of—*discretion* in speaking of their neighbours, and *modesty* in speaking of themselves!

London:

Printed by C. Mitchell and Co., 12 and 13, Red Lion-court, Fleet-street.

